The Journal of HEREDITY

REARED APART, BUT CAN YOU TELL THEM APART?

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Five Dionnes and How They Grew

Eugenics in the Colleges

Short-Toed Fowls

An Appendicitis Family

Coat Color in Horses

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The American Genetic Association

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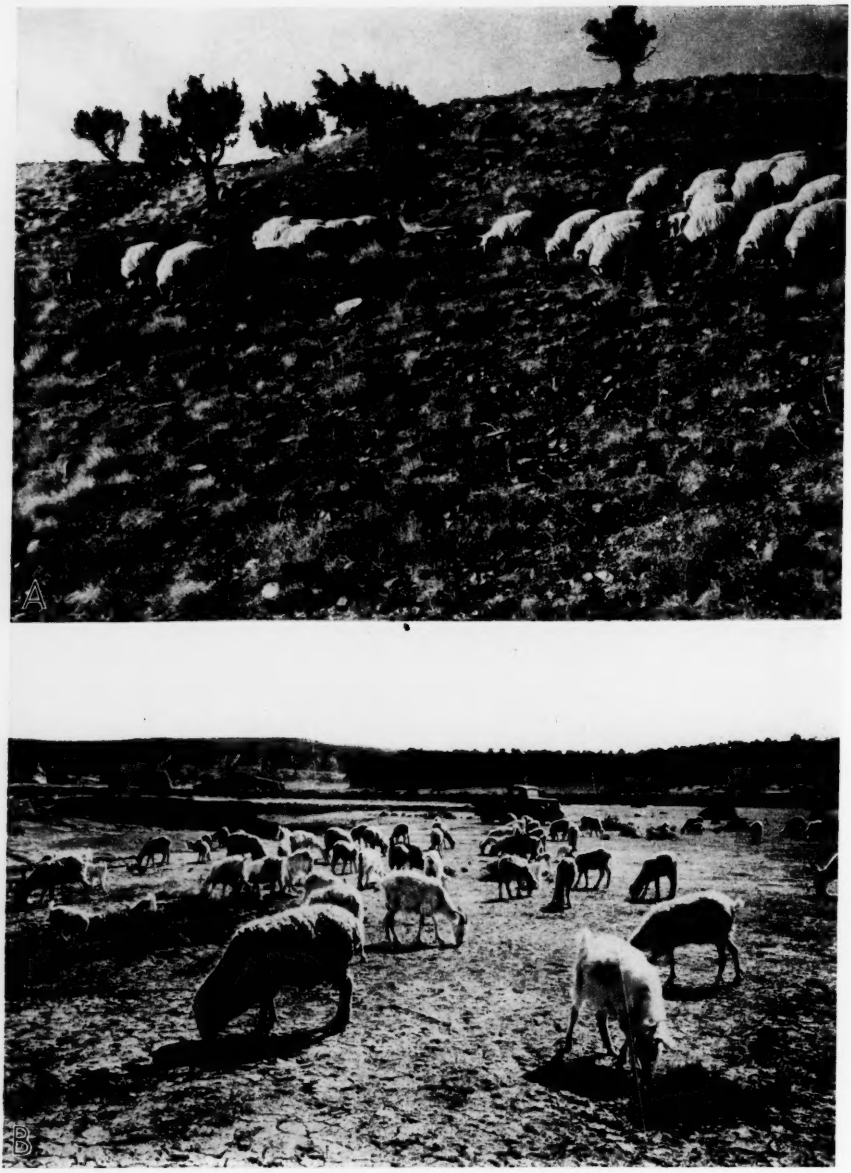


NAVAJO SHEEP

Frontispiece

A band of old type Navajo ewes (*A*), showing the long, coarse, hair-like fleece. These sheep, while a varied lot, have been adapted by generations of virtually natural selection to an arid environment, and produce wool excellent for weaving in the Navajo hand looms. *B*—A typical Navajo flock of today, consisting of sheep and goats, with the rams and billies running with the flock—a common practice among Navajo sheep owners. Introduction of many breeds in recent years and the absence of any consistent breeding plan has greatly increased the variability and reduced the quality of the sheep. To graft the valuable qualities of the old Navajo sheep onto some of the modern breeds, combining the advantages of both, is the aim of the present project. Photographs courtesy of Soil Conservation Service and Navajo Service.

IMPROVEMENT OF THE NAVAJO SHEEP

A Unique Breed and the Project to Improve It As Developed at the Southwestern Range and Sheep Breeding Laboratory*

CECIL T. BLUNN

Bureau of Animal Industry, U. S. Department of Agriculture

THE Navajo Indians are dependent on their flocks for the major portion of their livelihood. In the early days—before the railroads brought the competition of white settlers with better animals and more enterprising breeding and handling methods—the Navajo sheep gave their owners a satisfactory living. As a result of many years of poor herding practices and reverse selection, the sheep have deteriorated. In spite of some introduction of improved breeds, the wool and meat produced by the present Navajo sheep are inferior to that produced by sheep owned by neighboring white people.

The deterioration of the Navajos' sheep has long been recognized as a problem by the Government, as well as by others concerned with the well-being of the Indians. To improve these sheep the Southwestern Range and Sheep Breeding Laboratory was established in 1935 at Fort Wingate, New Mexico, through the cooperation of two Federal agencies, the Department of the Interior and the Department of Agriculture. In this laboratory the problems of genetics and animal husbandry will be integrated with studies of the various phases of weaving. The problems confronting the laboratory are a direct outgrowth of the cultural habits and tradition of the people.

The Navajo Reservation

Approximately 50,000 Navajos live on their reservation, an area of nearly 16,000,000 acres, located in northeastern Arizona, northwestern New Mexico and that portion of Utah lying south of the San Juan river.

The whole region is semi-arid. Desert vegetation is found at the lower elevations, giving way on the slopes to the woodland areas of piñon and juniper. On the higher mountains there are areas of commercial timber. In general, however, the plants of the area are limited to grass, browse, and weeds suitable for maintaining livestock. The naturally sparse vegetation has been depleted by severe overgrazing resulting in accelerated erosion. As a result the whole terrain is cut up by innumerable deep ravines and arroyos. Each heavy rain ravages the range lands, widening, deepening, and lengthening the gulleys.

The climate is very dry, with most of the rain coming in June, July and August. The fall months have a delightful climate, with warm days and cold nights the rule. The winters are usually quite mild and open. The spring months are characterized by violent dry, south and southwest winds with their attendant dust storms. The growing season is short, averaging about 155 days.

Life and Customs of the Navajos

The Navajos still live in primitive log and dirt structures called hogans (Figure 1). They have always been a semi-nomadic people, depending upon the country for subsistance. In the past they roamed over a greater part of the Southwest than is included in the present reservation. This mode of life and the scarcity of water rendered impractical for them the agricultural life led by their

*The author is indebted to Mr. J. M. Cooper, Director of the Laboratory, for his advice and criticism of the manuscript, and to Mr. R. F. Van Valkenburgh for his aid with the old files at Fort Defiance, and for the use of his personal reference files. Mr. H. T. King, Jr., of the Laboratory staff, also aided in the search of old documents and records.

NAVAJO HOMESTEADS

Figure 1

A—Shows an old-style Navajo "hogan", made of logs covered with earth. The modern hogan (*B*) is somewhat more pretentious. The photograph of the woman weaving was made about 1885, but the methods are exactly the same today. Samples of wool from old rugs and fabrics are being analyzed to determine how the wool type of the sheep has changed with the passing of the years. Photographs courtesy of Navajo Service, and of the Ben Wittick Collection, Santa Fe, New Mexico.

Pueblo neighbors. They did, however, raise some crops (maize, melons, etc.) in isolated spots where water was available. After they had acquired sheep and horses, they continued their nomadic life in order to find fresh pastures for their livestock.

Even though irrigation is making more tillable land available, sheep are still the chief source of Navajo income. Besides using sheep for food, the Navajos receive a cash return from the sale of wool in the spring, of feeder lambs in the fall, and from the sale of rugs woven from the wool.

Tribal legends, seasonal dances, and other ceremonials form an intrinsic part of the generally hard life of the Navajo. The artistic inclinations of the men are largely expressed through sand painting and silversmithing; while those of the women are expressed in the weaving of rugs. Many of the more wealthy Navajo women weave excellent rugs for pleasure, even though the monetary need is no longer a driving stimulus.

History of Navajo Sheep

Coronado, in 1540, brought the first sheep into what is now the United States. When he left Mexico in his search for the Seven Cities of Cibola, Hodge,[5] tells us that he took with him 5,000 sheep and 150 cows of Spanish origin, but of unknown breed. Some historians hold that these sheep were Merinos, but it seems more reasonable to assume that they were of the lowland Churro breed. At that time the Merinos were a Royal monopoly and were very highly prized. Such valuable beasts would hardly be used to provide meat for an army.

When Coronado quit the country in 1542, he left some sheep with the three priests who stayed on in New Mexico. These priests were either killed or forced to flee, and it is doubtful whether any of the sheep survived. The Indians were very unfriendly toward their Spanish conquerors, and probably destroyed everything which belonged to them. It has been suggested by Amsden[1] that some of the sheep might have been saved by the Pueblos, who combined weaving with agriculture, and who might have recognized the value of sheep as a source of meat and wool.

In 1598 the Spaniards again introduced sheep into the Southwest, and since then they have been ever-present and increasingly abundant. The exact type of these early sheep is not known but descriptions of their descendants after about 200 years[4] show that they were not of a Merino type at that time. Gregg states that about 1834 the wool was coarse, long, and practically worthless as an article of commerce.

The Navajos probably first acquired sheep soon after the Spaniards brought them into the country. The reports of the early Spanish governors record instances of raids by the Navajos and Apaches. Sheep, horses, and human captives were the main objectives of these raids. Strife between the settlements along the Rio Grande and the Navajos was reported later by the Mexican governors. Reprisals were made by the Mexicans, and treaties of peace were made, but were not lasting, for when the Navajos felt so inclined they again sallied forth on raids. No question of moral turpitude was involved in the theft of sheep and horses, nor in the taking of life, for the Indian considered it an act of bravery to risk his life on the warpath and any spoils obtained were fair booty.

The number of sheep in the Southwest during the latter part of the eighteenth and the first half of the nineteenth centuries can only be estimated. Referring to the period around 1835, Gregg states that some of the ricos (large, wealthy land owners) owned as many as 300,000 to 500,000 head. To the large totals of these herds, sheep owned by the Navajos must be added. The number of sheep continued to increase despite the constant friction between the Mexicans and the Navajos. Letterman[6] states:

"The number of sheep has been variously estimated, by those who have been among them, the highest estimate being 200,000, and the number is probably as near the truth as can be obtained. The wool is coarse and is never shorn. The sheep are in all respects similar to

RANGE UTILIZATION

Figure 2

The upper view shows an all too frequent sight on the Navajo reservation, where long-continued use of the corral adjacent to the hogan has resulted in the complete denudation of the surrounding area. During the time a hogan is occupied the sheep are returned to the corral every night. Thus for a period of from four to six months, the surrounding area is grazed over every day. Below is shown a range on which the grazing has been properly controlled, with the ground adequately covered, and erosion at a minimum. Photographs courtesy of Navajo Service, Window Rock, Arizona.

those raised by the Mexicans, occasionally one being seen with four horns. . . . . It is a great mistake to suppose that there is anything peculiar about Navajo sheep, for such is not the case." [The statement that the sheep were never shorn undoubtedly refers to the absence of set shearing practice. The wool was probably taken when needed for weaving.]

The constant strife between the Mexicans and Navajos continued after the United States took over the territory in 1848, for it made little difference to the Navajos who their nominal rulers were. They had long been accustomed to being "lords of the soil" and for centuries everyone had feared them. They resented the intrusion of hunters, miners, and stockmen into their homeland. The United States Army made peace pacts with small bands of Indians and attempts were made to look after their welfare. As before, the Navajo respected these agreements as long as they suited his purpose. That they continued to steal stock is shown in a report by Bowman[2], who investigated a claim for damages incurred through the theft of 4,000 sheep by Navajos in 1866. Internecine strife between the army and civilian leaders hampered the conquest of the Navajos.

After almost fifteen years of ineffectual bargaining with the Navajos, Brigadier General James W. Carlton was placed in command of the army in New Mexico. Carlton and his right-hand man, Colonel Christopher (Kit) Carson, were experienced Indian fighters. Their campaign against the Navajos began July 1, 1863. The Indians were ordered to come in and surrender. Men who failed to obey the order were to be killed on sight; women and children to be seized and held as captives.

Carson fought no pitched battles, but his troops kept the Indians moving from one refuge to another, and destroyed all crops and livestock in their path. So thoroughly was this done that by the summer of 1865 the Navajos were a broken people. Approximately 8,500 men, women and children, with 2,000 sheep were confined at Bosque Redondo (Fort Sumner) on the Pecos River. The campaign had been successful, with little loss of life on either side.

The Navajos were not an agricultural people, and it was soon evident that they could not be kept at Bosque Redondo. On June 15, 1868, a peace treaty was made whereby the Navajos were to be returned to a reservation in their former habitat.

Not being animal husbandmen, they were unaccustomed to caring for livestock. If their stock were not hardy and able to survive the rigorous conditions, they perished. Letterman, describing the sheep management practices of the Navajos, says:

> The males are permitted to run with the herd at all seasons, and the young consequently, are born in the winter as well as in the spring and autumn, and many die. For this reason, their flocks do not increase with the rapidity generally believed by those not much acquainted with these people.

Under such a system of management, survival of the fittest played a most important role. Had there been any Spanish Merinos present, the type would soon have been lost under Navajo management. With only natural selection and indiscriminate mating, any type which might have been fixed by the Mexicans was soon lost by the Navajos. The sheep degenerated to a "scrub" stock, albeit a very hardy stock able to fend for itself. Early Americans in the territory describe the wool as being coarse, long, and of inferior quality. Even the Mexicans used little of it for weaving.

Sources of Replacement Sheep

One of the articles in the treaty of peace signed at Bosque Redondo provided for the purchase of 15,000 sheep and goats by the army for distribution to the Navajos. Old army records and the contract for the purchase of these sheep show they were obtained from Vicente Romero of La Cueva, Mora County, New Mexico. They were the native Mexican sheep, and were to be brought to Fort Defiance in the fall of 1869, but delivery was delayed until the following spring.

About 7,500 Navajos gathered at Fort Defiance for the first issue of animals. Each man, woman and child was given two sheep or goats, later each was is-

sued another sheep from a purchase of 10,000 animals made in 1872.[7] These first issues, along with the animals left on the range by Kit Carson and his men, and the few brought back from Bosque Redondo, again started the Navajos in the sheep business. If any other sheep were purchased between 1872 and 1880, a diligent search has failed to reveal the source of the funds or the delivery of the sheep.

The first efforts to improve the type and quality of the Navajos' sheep were made by D. M. Riordan,[8] U. S. Indian Agent, 1882-1884. In his first monthly report, Riordan states:

> The sheep owned by the Navajos are "scrubs" . . . . The fleeces that now shear 1 and 1½ pounds ought to be made to shear 5 and 6 pounds. The effects on the Indians would be to make them grow a longer and better wool, to clean it better, to take care of the good stock and use the "scrub" for food, to learn that 1,000 highbred sheep require no more water and grass than 1,000 "scrubs" while they will produce five times as much in value.

Riordan follows this with a plan whereby improved sheep would be introduced and *earned* by the Indians. After authority had been given, 75 selected bucks were purchased at $25 each delivered at the Agency. These rams were $\frac{15}{16}$ pure California Merinos and sheared from 15 to 25 pounds. The bucks arrived during a heavy snow storm about the end of April, 1884. They were kept at the Agency for some time before being sent to the sheep corral at Washington Pass, north of Fort Defiance. Everyone expected much good to come from their use, but no mention is made in Agents' reports of the bucks being loaned to the Indians for service in their flocks, or of any other use being made of them.

J. H. Bowman succeeded Riordan as Agent, July 1, 1884. Like Riordan, Bowman thought little of the Navajo sheep:

> While they have a great number, they are of very inferior quality. The sheep will not average 40 pounds dressed, and the yield of wool is not more than one pound per head, and that of the very coarsest quality, only used for carpets . . .

Bowman sought authority to add 800 to 900 ewes of similar grade to the 75 bucks purchased by Riordan. He said:

> The offspring of these improved sheep issued judiciously to the most deserving Indians would naturally create a desire for improvement in their stock, would double the revenue in a short time.

Apparently two plans were being considered at that time. Under one plan, a breeding flock would be maintained by the Agency, the offspring to be issued to deserving Navajos. The other was to purchase rams and place them in the larger Navajo flocks.

In a letter to the Commissioner of Indian Affairs, dated October 10, 1884, Bowman made the first recommendation for the use of Cotswolds instead of Merinos. In the same letter, the first mention was made of the unsuitability of Merino wool for hand spinning and weaving. Whether Cotswolds were introduced at that time is not now known. Authority was repeatedly requested for their purchase, but no records of actual purchase have been found.

Although no unified breeding program has been followed, many improved breeds of sheep have been introduced on the reservation since the beginning of the present century. All the Indian Agents and their assistants have imported rams of one or more improved breeds. In addition to the sheep introduced by government agents, many private importations have been made by traders and others interested in the welfare of the Navajos. Each made an individual and sincere effort to improve the sheep of the Indians, but without regard for the fundamentals of animal breeding. Further, most of the superintendents remained only long enough at one point to start a breeding program before being transferred elsewhere. Their successors usually dropped the programs already under way and instituted plans of their own, often using a different breed of sheep. The multiplicity of sheep types introduced without any organized plan has resulted in a heterogeneous and very inferior sheep.

Present Navajo Sheep

On the reservation today, two distinct

types of sheep are found (Frontispiece). The old-type Navajo sheep are encountered only in a few regions farthest removed from white influence. These are clean legged, long, upstanding and narrow bodied, like the Churro and Tarhumara, found in Mexico today.[9] Their fleece consists of a long, coarse outercoat and a short, fine undercoat, with many kemp and medullated fibers which are characteristic of unimproved wool. It is extremely open and contains relatively little grease. The shrinkage, although extremely variable is lower than that of medium wool from improved sheep. Most of the shrinkage is occasioned by the accumulation of dirt and vegetable material in the wool. The sheep are very hardy, and the ewes are good mothers. Many of the rams possess two well developed horns, and occasionally four; some of the ewes are also horned.

The majority of the sheep now on the reservation show the results of the many crosses with improved breeds. In contrast to the old-type Navajo sheep, the fleeces have an extremely short, fine staple; the grease content is higher, and the shrinkage greater. Most of the sheep are a poor grade Rambouillet, although it is possible to pick out many individuals showing close resemblance to other breeds.

Navajo Management Practices

During the past 75 years there has been no marked changed in the Navajos' method of handling sheep. The herding, which is still the task of the women, children, and old men, is habitually done from the rear of the flock. This tends to keep the animals moving more rapidly than is desirable, especially in mixed bands of sheep and goats. Frontispiece *B* shows a typical Navajo flock. The goats are always in the lead and the sheep, in their efforts to keep up with the leaders and away from the herder and dogs in the rear, are forced to graze as they move along. Rarely is the flock held in check so that it may spread out and graze quietly. The animals frequently suffer during the dry periods when they are forced to go two or three days without water.

The Navajo reservation is greatly overstocked, with more than 720,000 sheep units on an area which the Soil Conservation Service estimates as capable of carrying only 537,000 sheep units (horses and cattle are converted to sheep units on a basis of 1 to 5 and 1 to 4, respectively). Overgrazing, poor herding practices and scant water have resulted in an inefficient use of the range. The vegetation around the water holes has long since been eaten and tramped out. The old Navajo practice of corraling at night and grazing out from the corral has also denuded the land around the hogans. For constantly increasing distances away from the bedground the range is severely overgrazed, if not by an actual overstocking at least by a prolonged over-use. A new corral is used only when the family moves which may be as infrequently as two or three times a year. At the same time some of the more distant parts of the range may be under-utilized. The same corral is used each time the family occupies the adjacent hogan, until no more forage is available in its vicinity. The long continued use of the corral gradually raises the ground level inside the fence. Thus many old corral sites are plainly visible in the centers of barren areas. Figure 2*A* shows one of these old corrals, while Figure 2*B* shows range on which the grazing has been controlled properly.

The Navajos rarely follow any breeding system and persist in allowing the rams to run with the ewes throughout the year. A ram is used until too old to be of any further service with little regard to the effect on the flock. Lambs are born from November to July, which results in an inordinately high death loss, especially during the winter.

Sheep and Navajo Economics

The Navajos raise approximately 300,000 lambs each year. Of this number between 80,000 and 100,000 are sold as feeders in the fall; the rest remain on the ranges and are used for

food and replacements. The Navajos consume large quantities of mutton, which they prefer to beef, pork, or poultry, but no consumption figures are available.

The feeder lambs lack uniformity of age, size and development on account of the system of lambing. Since the green feed dries up before the late spring lambs are well started they are apt to be stunted. Range conditions are not conducive, even under the best management practices and with good type sheep, to the production of well finished lambs. Under the Navajos' untrained hand and with the poor type of sheep which they possess, only a very inferior lamb can be expected.

Each fall the Navajo takes his feeder lambs to the trading post where they are weighed, sorted and culled by the trader. Any animals falling below minimum weight and size standards are rejected and returned to the Indian. These standards may vary from year to year, depending on the demand for feeder lambs. Some of the smallest lambs are the youngest, but a large number of them are those which because of poor feed, genetic constitution, and a combination of other factors, failed to grow. The lambs finally bought by the traders eventually find their way into feed lots in the adjoining states and in the Middle West.

Many of these cull animals rejected by the trader are used by the Indians to maintain the size of the breeding flock. Many male lambs are not castrated so that sire selection is at a minimum. This practice of maintaining the flock through breeding from unsaleable animals is one of the main causes of the degeneration of the Navajo sheep.

When one considers the range conditions and Navajo herding practices we see that practically all the requirements for the production of good quality wool are disregarded. The method of shearing adds nothing to the quality of the poorly grown wool. In former years an old knife or a piece of sharpened tin was used to hack off the fleece,[8] which realized only about half the available wool as a usable or saleable product. Although hand shears are now used, the animals are often thrown in the dirt of the corral and their feet tied together. After shearing is completed the untied fleeces, irrespective of color or quality, are sacked and taken to the trader, where the price received per pound reflects the poor growth and preparation of the wool. Eventually it finds its way to eastern markets, where it competes poorly with other domestic wool.

Not all the wool, however, is sold. Some women sort out and retain that which is the most desirable for weaving. Occasionally traders, too, save out the best weaving wool and sell it back to the weavers. During the winter the women make this wool into rugs.

Navajo Weaving

Amsden suggests that the Navajos probably learned to weave from their Pueblo neighbors, who made cotton fabrics long before sheep were introduced by the Spaniards. The first Navajo weaving was probably done some time in the latter half of the Seventeenth Century, after they obtained sheep from the Spaniards. At first they wove only clothing (Figure 3), but in a short time they began weaving blankets for barter. Today rugs are the principal product.

Entire books have been devoted to the subject of Navajo weaving, which is unique among the handicrafts. With the simplest of hand instruments, the raw wool is converted into a beautiful and useful commodity. Figure 1*A* shows a woman at her loom.

Rugs and blankets yield 26 per cent of the total livestock income of the Navajos.[3] Since the major part of the weaving is done at a season when other income sources are almost wholly absent, the importance of weaving is not reflected in stating the income as a mere percentage of the annual total. In addition, the weaving of rugs affords the women a means of creative expression, just as knitting, crocheting, and lace making afford the satisfaction of artistic expression to women of other nationalities.

Sheep Research in the Navajo Area

An exhaustive survey of the needs of

THE NAVAJOS FORMERLY WOVE CLOTHING FABRICS

Figure 3

And old photograph of a Navajo couple showing the hand-woven dress worn by the woman. The blankets are of the commercial "Pendleton" variety, but the dress represents a type of weaving now forgotten, rugs being the main product of the Navajo looms today. Courtesy of the Ben Wittick Collection.

the Navajo people made in 1934 estimated that sheep and wool were the chief economic assets of the Indians; and that the old-type Navajo sheep was essential to the rug and blanket industry. Following this survey, officials of the Bureau of Indian Affairs and the Bureau of Animal Industry conferred to find a way to aid the Navajos.

These studies disclosed the urgent need for establishing a laboratory to work on the problems confronting Navajo and other Indian sheep raisers of the Southwest. Four broad objectives were recommended for study: (1) to determine the type of sheep best adapted to the ranges of the area and the needs of the people and then (2) to develop this type of sheep; (3) to conduct work on flock management and range improvement to determine the best methods of handling sheep and range to secure maximum production, and (4) to assist, wherever possible, in the education of the Indians so that they might use their resources of sheep and range to maximum benefit. While wool improvement was the major objective the need to develop as good a mutton conformation as consistent with conditions on the reservation was recognized.

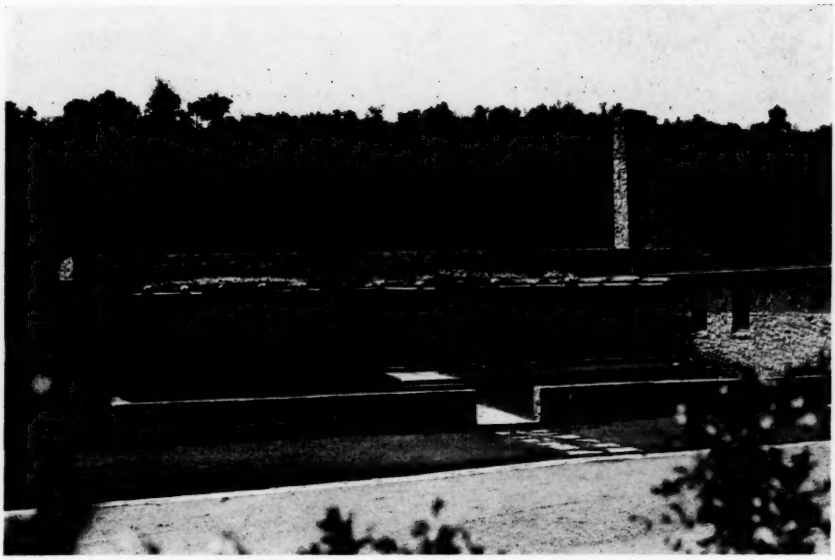
Recommendations of the conferences were accepted by the Commissioner of Indian Affairs, and Congress made an appropriation available on July 1, 1935, to set up the laboratory. In the meantime, through the efforts of specialists in the Bureau of Indian Affairs and the Bureau of Animal Industry, the foundation was laid for an organized study in wool technology and sheep breeding, and detailed plans for a laboratory were drawn. This eventuated in a formal cooperative agreement between the Department of the Interior and the Department of Agriculture. The Bureau of Indian Affairs in the former Department undertook to enter into a joint project with the Soil Conservation Service and the Bureau of Animal Industry in the latter Department in developing this laboratory. In December, 1935, the Secretary of Agriculture formally approved this project under authority of the Bankhead-Jones act.

According to this agreement, the Bureau of Indian Affairs furnished the sheep, laboratory buildings, quarters, and range, and provided for the maintenance of the sheep, and the general operating costs. The Soil Conservation Service agreed to give advice concerning the stocking and carrying capacity of the range; to investigate erosion control, range revegetation, and related problems. The Bureau of Animal Industry agreed to provide specially bred or specially selected breeding sheep for the research work, in addition to those furnished by the Bureau of Indian Affairs; and to meet the expenses of the breeding and wool research laboratory.

The original equipment furnished by the Bureau of Indian Affairs included about 19,000 acres of range land, a laboratory building and quarters, about 800 old-type Navajo breeding ewes, and about 20 rams, along with equipment and personnel for their maintenance. The Bureau of Animal Industry furnished the improved rams for the cross-breeding studies; equipped the laboratory with research apparatus, and furnished the technical personnel.

The Research Program

The research work up to the present time has been devoted chiefly to the determination of the type of sheep best suited to thrive on the arid ranges of the region, and to produce wool and button in accordance with the objectives outlined. Coincident with these studies the laboratory will endeavor to purify and improve the old-type Navajo sheep and to synthesize a new strain by crossing these sheep with some of the improved breeds. Because of the ability of the old-type Navajo sheep to withstand adverse range conditions and poor management, it is believed important to retain these characteristics. An effort is being made to increase uniformity and wool quality and to provide a more stable breeding strain. Selection is being directed to reduce kemp, medullated and long coarse fibers and at the same time



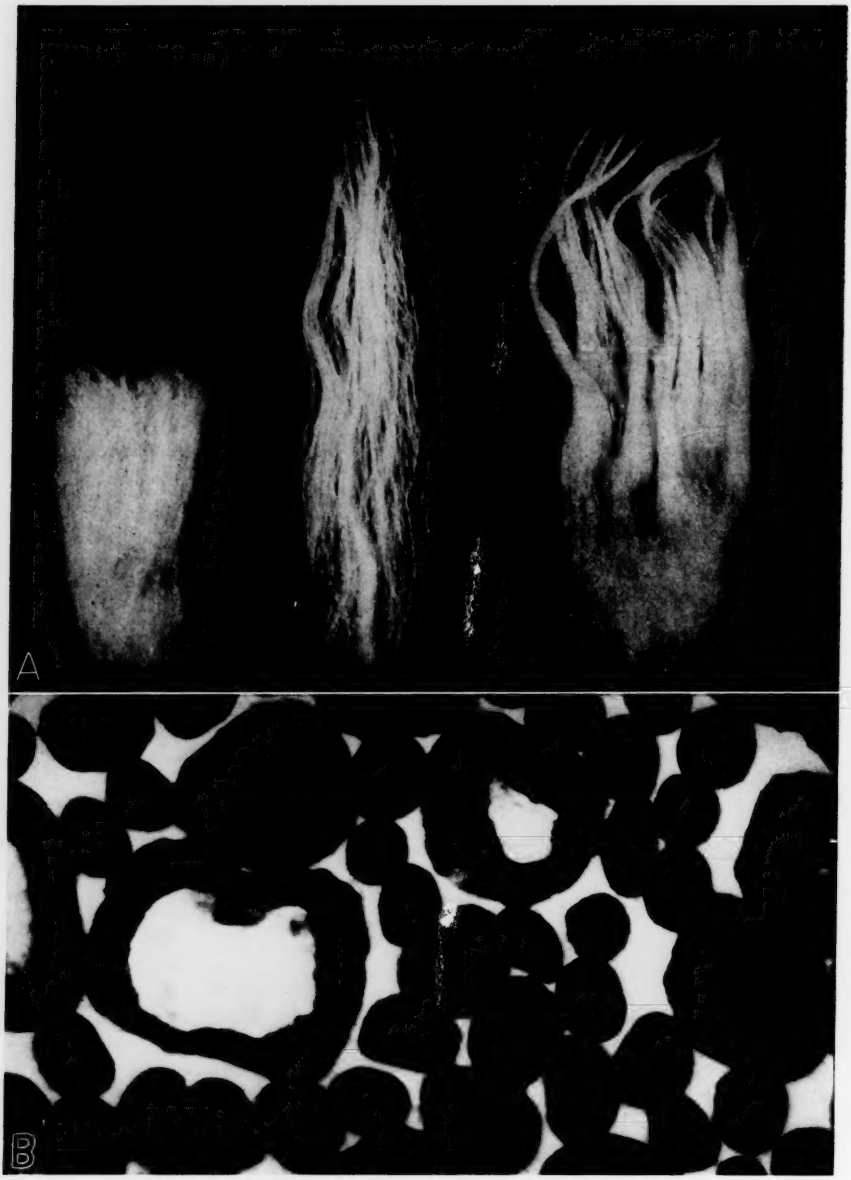
THE SOUTHWEST RANGE AND SHEEP BREEDING LABORATORY
Figure 4

Located at Fort Wingate, New Mexico, this laboratory was established in 1935 to study the problems of sheep breeding and sheep husbandry in the Southwest. It is operated jointly by the U. S. Indian Service and the U. S. Department of Agriculture.

to retain the hardiness and good mothering qualities of these sheep. At present inbreeding is being held as low as possible and improvement is being made by rigid selection of the offspring. As soon as a desirable type appears in any group, sire families may be set up and the type fixed as rapidly as possible by close inbreeding.

Crossbreeding is being undertaken to incorporate some of the desirable characters of the improved breeds into the old-type Navajo sheep. This will synthesize a new strain, combining the most desirable characters of the improved breeds and the Navajo. The hybrids should have better conformation, be larger, and have better and more uniform wool. Since many undesirable fleece and body characteristics have been shown to be quantitative in inheritance, mating Navajo ewes with rams of improved breeds should aid in eliminating such characteristics. The offspring will not be entirely free from these characters, but dilution will make them progressively less serious.

As yet the breeding work has not progressed far enough for a fixed breeding plan to be outlined and followed. Since the mode of inheritance for most of the characters involved is complex and as yet not entirely understood, the exact plan of the matings for each year's breeding depends largely upon the offspring obtained. Determinations made in the laboratory indicate that the Romney and Corriedale breeds possess many of the fleece and body characteristics desired in a sheep type for this region. Rams of these two breeds are therefore being used at present. The F_1 crossbred animals show wide variations in fleece character and quality, but they show definite improvement in these qualities over their dams. The F_1 ewes have been backcrossed to old-type Navajo rams. This was done so that the good qualities of the Navajo sheep would be intensified, even though some fleece qual-



SAMPLES OF NAVAJO WOOL
Figure 5

Length separation of Navajo wool (*A*). and photographs of cross sections of such a sample (*B*), to show great variation in fibre. In *A*, part of the sample at the right has been separated into two components — long coarse outer coat (center), and a fine undercoat (left). The former frequently attains a length of ten inches; the latter, rarely of more than four inches. In the section (500 ×) the very fine fibres constitute the undercoat, while the large fibres, both medullated and with a solid center, make up the kemp and the outer coat.

ity might be lost. No close inbreeding or line breeding program will be set up until laboratory determinations show that the desired qualities of both wool and body conformation have been reasonably well attained.

Samples of wool are taken from all the sheep in the flock. Tests on the cross-sectional fineness and variability, length, strength, and shrinkage of these various wool types are made in the laboratory (Figure 5). Samples of various type wools are also woven into small experimental rugs by Navajo weavers to afford a practical comparison of the suitability of the various wool types for hand spinning and weaving. Checking these rugs with the results of the laboratory tests gives a more accurate determination of the ideal type of wool for handicraft use.

To provide a further check on the laboratory tests an agreement was made with the Federal Prison Industries to have wools of the various types partially processed by machine. These are then finished by the Navajo hand methods, and the results compared with totally hand-made products. The suitability of these wools for fabric manufacture by machine processes is also being tested.

Growth studies are being pursued on both the typical Navajo and the cross-bred sheep and feeding trials are being conducted to determine which sheep will make the most economical gains in the feed lot. This is an essential point when we recall the importance of feeder lambs in contributing to the Navajo income.

To determine the type of wool used in weaving blankets at various periods, wool samples of very old blankets and rugs have been obtained from museums and private collections and are being tested in the laboratory for fiber fineness and variability. The results will be correlated with the data obtained on the breeding flock and thus will serve as a further check on the direction the breeding program should take.

Discussion

The primary purpose of the laboratory is to undertake the improvement and the synthesis of a type more suited to range conditions and to the needs of the Navajos. At the same time the laboratory is to serve as a coordinating agency in directing the general improvement of all sheep on the reservations in the Southwest. Steps are being taken to increase and preserve the range land resources of the Indians, and to improve the methods of handling sheep.

Since Rambouillet breeding predominates in most of the sheep, all rams introduced under the present policy must be Rambouillets conforming to certain fleece and body standards. This practice, which will increase uniformity and quality of the sheep and wool for the market, will be continued until a type has been evolved more suited to all the needs of the people and the area.

It is recognized that Rambouillet type wool is unsuited to the hand spinning and weaving of the Navajos. Because of this, the present policy is liable to be criticised. But it must be remembered that weaving constitutes only about 26 per cent of the total Navajo livestock income. Therefore, any steps which will tend to increase the quality of market wool and feeder lambs should be encouraged, for these commodities make up a far larger proportion of the Navajos' livestock income.

In order to maintain a supply of wool more suitable for weaving, an exception to the present administrative policy is being made in a small isolated part of the reservation where the breeding of the old-type Navajo sheep is encouraged.

In an effort to bring about improvement in the industry, pastures have been established to care for rams used by the Navajos. Rams are issued to the Indians only at the proper breeding time for their locality. More vigorous rams, an increased lamb crop, an improvement in type, and more uniformity in age and size of feeder lambs have been the immediate benefits from this control. The Indians are being encouraged to board their own rams in the government pastures as well. The need for early castration of ram lambs is also being emphasized.

While the present policy is too new to show definite results, the trends are encouraging. Through schools, extension service and demonstrations every effort is being made to teach the Indians that range and sheep are interdependent; that if damage is done to one it will be reflected in the other.

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The Dalmatian Psyche

To the Editor:

Each issue of your Journal has something that especially fascinates me. This time it is Keeler's & Trimble's article "Inheritance of Position Preference in Coach Dogs."

They make the point that "an untrained animal (Dalmatian Coach Hound) may occasionally break from his leash when a carriage goes by, not to chase the horses, but to run happily under the carriage where he feels instinctively that he belongs. This instinctive reaction. . . . ."

The interpretation of this "instinct" ought to be more interesting and instructive than the description which these authors provide.

I suggest that the primary basis of the Dalmatian's behavior is kinetic. These dogs seem to love rapid movement more than do other breeds. They race *along side* of a saddle horse with as much apparent pleasure as *behind* a harness horse. They also show the same glee when they ride *on* a horseless fire truck.

There may also be an auditory factor in their behavior—the vibration of wheels, the thumping of hooves, the squeal of sirens.

As for their love of horses, this may be a special case of gregariousness. Or the dogs may like the smell of horses. More likely it is the result of association. Trainers of race horses successfully use chickens and goats to keep their horses company.

Myron Weiss

130 W. 57th St., New York City

To this Dr. Keeler replies:

Dr. Myron Weiss may have a good hunch in his idea that the basis of the Dalmatians' coaching behavior is kinetophilia. But there is a great deal more to it than just that.

Dr. Trimble and I only mentioned manshyness as a probable factor in coach running ability because we did not dare attempt a complete psychiatric analysis. Again, our example of the pup under the wheelbarrow suggests additional factors in behavior.

The Dalmatian is in general, "a hale fellow well met," just bubbling over with enthusiasm and not very well controlled in that he is ready to climb all over his newly made human friend as soon as the friendship is proffered. I am told that often he is a poor watch dog and is so delighted with an opportunity to meet a burglar, and lick his hands and be fondled, that in return he lets the burglar walk away with the whole house in his bag. The Dalmation is an incorrigible extrovert and wants to be in on everything that is being done, especially if there is distance to be covered and he does not care about the purpose of the trek so long as he is allowed to accompany the party.

A lifetime could be devoted very profitably to comparative psychology of canine pure breeds before we would be certain of the actual components making up such phenomena as the coaching trait in Dalmatians.

Clyde E. Keeler

A FAMILY PEDIGREE OF APPENDICITIS

SISTER M. FLAVIA
College of Saint Francis, Joliet, Illinois

HUMAN traits which fail to show a simple manner of inheritance are frequently observed to "run in families." Baker[1] and others have noted that appendicitis occurs more frequently in some pedigrees than in others. This paper presents a group in which the incidence of appendicitis is at least twice that in the general population, and includes two sets of cousins in which every person has had his appendix removed.

The first three generations of the group studied are all German or of German descent. I-1 was brought by his parents from Germany to Chicago in 1854 when he was four years old. When he was about thirty years of age he moved to Kankakee County in Illinois where he married I-2, also of German descent. All the members of generations II and III were born in this same part of the country. Some of generation III later married into families of French or Irish descent.

All four generations are typical middle class citizens living in well-built and well-furnished homes with plenty of wholesome food. Their homes are either in the country or in small country towns. Practically all the members of generation III were born and reared during the "days of abundance," previous to the depression. It is precisely in this group that the greatest number of cases of appendicitis occurred.

Among the descendants of II-1 and II-5 the incidence of appendectomy has been 100 per cent. What is still more significant is the fact that the sibs included in these two groups have the same ancestors, as II-1 and II-5 both married members of the same family.

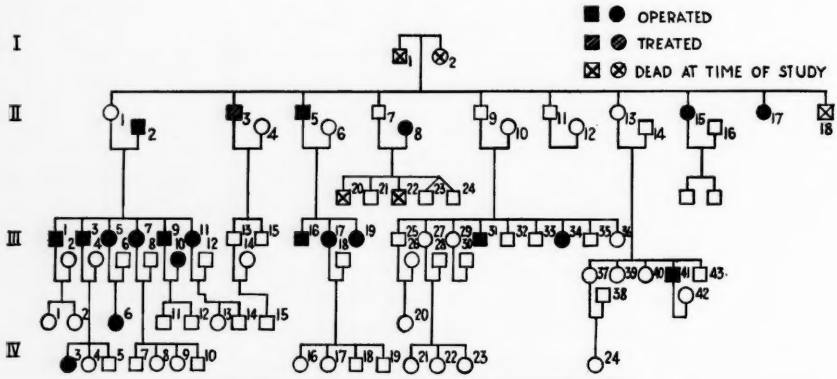
Both families lived in the country in very comfortable homes near the homes of their parents. The environment of the two families was very similar until the children of II-5 were of grammar school age, when he moved his family to Missouri. As the methods of diagnosis of diseases during the last half of the previous century were far inferior to our present methods, it is impossible to give a record of the ancestors of I-1 and I-2.

Details concerning the nature of attacks and ages when operations were performed on the affected individuals in the pedigree chart are presented in Table I. II-3 has not yet had his appendix removed, although his doctor states that he had a typical attack of appendicitis in 1892 when he was 16 years of age. The appendix of II-5, when removed, was found to be greatly deformed.

The most serious infections were those of III-3 and III-16. III-3's first attack occurred when he was 27 years old. One month later he had an acute attack which required an emergency operation. The appendix was found to be ectopic, lying in the ventral part of the abdomen under the mid-line. The portion of the small intestine lying posterior to the infected appendix was perforated and had to be removed with the appendix. Through the skill of the country docter, the subject was saved.

III-16 became ill when a senior student at a medical school, and diagnosed his case as appendicitis ten days before the medical staff pronounced it typhoid fever. After six weeks' illness the patient was considered "out of danger" by the attending physicians. Two days later a severe attack made surgery necessary, revealing a complication of infections in the appendix and the surrounding organs. The patient died on the third day after the operation.

III-34 was the youngest individual in the pedigree to have an operation. An



A FAMILY PEDIGREE OF APPENDICITIS

Figure 6

Among the children of II-1 and II-5 the incidence of appendicitis has been 100 per cent. II-2 is the brother of II-6. Eleven progeny having the same ancestors had appendicitis. The fourth generation is very young, and other cases may yet occur.

acute attack when she was 12 years old required an immediate operation, revealing an abscess and the appendix atrophied. The abscess was drained. The attending physician anticipated that a second operation would be necessary, but to date she has had no further attacks.

All of the other cases listed in Table I had one or two attacks followed by a successful operation.

The members of generation IV are still quite young, not one of the 24 having as yet reached the age of 20. Nevertheless IV-6, who was born in December, 1923, has had four minor attacks during the past two years and recently an acute attack, followed by successful operation. IV-6 has also been operated upon, following a single acute attack.

Discussion

In making a summary of all the operations studied, the age factor is somewhat remarkable. No cases in the extreme limits have been found. All 21 cases are recorded within the age limits of 12 years to 38 years of age. Nine cases occurred between 12 and 20 years of age, nine between 20 and 30, and four between 30 and 38. These figures correspond with the report published by Gross.[3] She states that the average age of all victims of appendicitis is 25 years.

Woods[4] reports in his survey of Philadelphia that during 1931, ten per cent of all operations performed were done on the appendix. In eighty hospitals in Pennsylvania, not including the cities of Philadelphia and Pittsburgh, 16 per cent of all surgical cases were appendicitis.

TABLE I. Nature of attack and age at time of operation or treatment for appendicitis of affected individuals in the pedigree chart.

Individual	Nature of attack	Age at time of operation
II-2	chronic	32
II-3	acute attack treated	16
II-5	several acute attacks	25
II-8	acute	38
II-15	several acute attacks	17
II-17	chronic irritation	38
III-1	acute, 3 years before operation, several minor	23
III-3	acute, month before operation	27
III-5	one acute attack	15
III-7	acute, 6 months before operation	34
III-9	one acute attack	21
III-10	several attacks	21
III-11	one acute attack	17
III-16	ill six weeks before operation—died.	24
III-17	one acute attack	19
III-19	several attacks	28
III-31	one acute attack	22
III-34	one acute attack	12
III-41	one acute attack (ruptured)	24
IV-3	acute	17
IV-6	several attacks	16

In a nation-wide U. S. Public Health Service Survey a report shows that one per cent of all population had appendicitis. "The prevalence of the disease is increasing each year." According to the pedigree chart, the seven cousinships in generation III had a record of approximately 30 per cent of appendectomies.

All the members of the group studied are well known to the author and the data for the appendectomies have been carefully collected from members of the immediate families. On account of the great number it would be impossible to collect a complete record of all the new members who have entered the family through marriage.

These data seem to confirm a former conclusion[2] that "It should be borne in mind that it is very unlikely that appendicitis is inherited 'as such' as is the case with blond hair . . . but probably as a predisposition toward inflammation of this superfluous portion of the human anatomy."

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A COLCHICINE CHRONOLOGY

To the Editor:

As you may remember I have written to you at the beginning of this year suggesting, amongst others, that the pioneer work on the effects of colchicine in plants, with which your Journal seems to have credited American workers, was in fact initiated in Belgium: by the writer and his collaborators (1936), and in France: by Gavaudan and co-workers (1937).

In reply you were kind enough to offer (on March 7th 1939) to publish my rectifications in an early number of the Journal of Heredity, together with abstracts of the earlier European discourses on the subject to which I have drawn your attention.

Until recently I have not attached enough importance to the publicity of these rectifications to trouble you in urging their publication. But now, judging from what I have heard and seen at the seventh International Genetical Congress (Edinburgh, August, 1939) and also because of the extraordinary general interest shown for colchicine induced polyploidy, I have come to the conclusion that a clarification of the situation would be desirable after all.

I have come to this conclusion because the general disregard shown at the "Colchicine Conference" of the Congress for the above pioneer work and its developments—as modest as these may be—seemed to me symbolical of the widespread indifference for the biological, physiological and pathological implications of colchicine induced polyloidy. This is why, in spite of the extraordinarily large attendance, and in spite of the acuity of observation and depth of analysis displayed on this subject in such papers as those of Professor Blakeslee or Professor Stomps, no synthetic view of the manifold—and some genetically equally suggestive—actions of colchicine has emerged. And thus, in splendid isolation suspended betwixt earth and heaven, like the coffin of Mahomet, *polyploidy* was offered alone to the worship of the conference.

Yet, while there is nothing further from my mind—and for a very good reason—than to minimize the importance of this discovery, it must be admitted that colchicine is not a Miraculous Presence suddenly revealed,—between longitudes 65 and 125 west of Greenwich. It is the result of more than two decades of laborious and steadfast work consecrated by Professor A. P. Dustin and his collaborators in Brussels to the discriminating analysis of the karyokinetic and karioclasic effects of a long list of "nuclear" poisons in relation to pathological conditions in animals. In the course of these investigations, which lead amongst others to the fruitful determination of *"Stathmokinesis"* Colchicine was only an incident, the drug being, although more potent, by no means more *specific* for the induction of stathmokinesis, than some arsenic compounds previously tried. It is in consequence primarily to the discovery of the principles underlying stathmokinesis, and to the deductions drawn therefrom by Professor Dustin himself, that the manifold applications of colchicine in problems of *animal* physiology and pathology are due (see the enclosed symposium on "Colchicine and Colchicine effects").

As to the discovery of the polyploidizing effects of the stathmokinetic action of colchicine in *plants*, with which your Journal is more specially concerned, the roads of approach of this discovery and the vistas opened by it by the writer and his collaborators may perhaps be outlined more explicitly together with the exact dates of their publication.

I. *February, 1937.*

(a) Discovery of the *analogies* between certain *stathmokinetic responses* in the meris-

tematic regions of wheat seedlings treated with colchicine and the germinative zones of animals similarly treated;

(b) *"Bulbons hypertrophy"* of root-tips and hypocotyle (this last being, according to Blakeslee, the more important for the obtention of polyploid mutants);

(c) The above *swellings* (and other growth effects) having been interpreted as *similar* to those induced by *B. tumefaciens,* known as *"Phytocarcinomata"* (or "crowngall"), a first *attempt* was made to *influence their development* by the simultaneous application of Viscum preparations, previously shown by the writer to be "carcinolytic" in plants, by:

L. HAVAS: *Nature,* vol. 139, p. 371, February 27, 1937 (see also for application in animal essays: *L. Havas and E. Gal, Nature,* vol. 141, p. 284, Feburary 12, 1938).

II. *March, 1937.*

Confirmation of the above findings by *cytological analysis* (in wheat and bulbous plants) as shown by the following quotations: ". . . . *profound alteration of the achromatic figure,* irregular distribution of chromosomes around a granulated acidophile central mass; complete inhibition of polar migration; *inhibition of spindle formation,* no phragmoplasts; fairly rapid reconstruction of nucleus by the chromosomes, but these nuclei are monstruous, resembling those of megacaryocytes" . . . . "hyperchromatic, plurinucleolated and pluri-lobed nuclei, *resembling very distinctly the atypical elements of malignant tumours,* etc." by:

A. P. DUSTIN, L. HAVAS *et* F. LITS. *C. R. Assoc. Anatomistes,* Marseille, 21 Mars., 1937. (The above interpretations of the swellings induced by colchicine were confirmed later by Kostoff, Levan, Lefevie, Chouard, etc.)

III. *June, 1937.*

Confirmation of the above findings and discovery of the *doubling of the number of chromosomes* in *Allium cepa* treated with colchicine, by:

P. and N. GAVAUDAN and N. POMRIAS-KINSKY-KOBOZIEFF. *C. R. Soc. Biol.* 125:705, June 26th, 1937.

IV. *September, 1937.*

Publication of the now famous communication of A. F. BLAKESLEE on the *doubling of the number of chromosomes* in plants by chemical treatment (colchicine), by:

A. F. BLAKESLEE. *C. R. Acad. Sci.,* 205: 476. 13 Sept., 1937. (This publication was followed in the U. S. and in Europe in quick succession by so many others that their list cannot be given here.)

V. *July, 1937.*

The above findings having confirmed the initial interpretation (I and II) of the pathological nature of the swellings caused by colchicine, the *curative effects* of the drug were tried on the *experimental "Phytocarcinomata"* of the tomato. This attempt was motivated amongst others as follows:

(a) Various carcinogenic agents of animals (amongst which several chemically related to colchicine) were shown to produce pathological structures similar to plant tumours ("crown gall," "phytocarcinomata") in a great many plants (KOMURO, BLUMENTHAL, LEVINE, HAVAS, etc.)

(b) On the other hand, some of these chemicals were shown to have curative effects upon certain malignant tumors of animals (HADDOW and ROBINSON, etc.)

(c) Some similarly encouraging curative effects of colchicine having been reported in animals (DUSTIN, LITS, AMOROSO, PEYRON), it seemed worth investigating whether analogous results could be obtained in plants.

(d) The *property of colchicine to inhibit the development of the experimental tumours of the tomato was indeed shown,* and confirmed in a still larger measure in many other plants by Brown and Solacolu et al.

L. HAVAS: *Nature,* vol. 140. p. 191, July 31, 1937; also vol. 143, p. 789, May 13, 1939.

L. HAVAS: *Bull. Assoc. Franç. pr. l'Etude du Cancer,* T. 26, No. 6, December, 1937.

N. BROWN: *Phytopathology* 29, No. 3, p. 221, March, 1939.

TH. SOLACOLU: *C. R. Soc. Biol.,* Paris, 130, 1148, 1939.

VI. *July, 1937.*

A *"hormonal" mechanism of action of Colchicine in plants* was suspected right from the beginning on account of certain of its *growth effects* (I, II) strikingly *similar* to those exercized by the *plant hormones,* Heteroauxine and Vitamine *C* (HAVAS, V. HANSEN, HAVAS and GAL., etc.) and also by certain *animal hormones* when administered to the same test plants (HAVAS and CALDWELL, etc.). It was also demonstrated that tumours similar to those produced by both *colchicine* and the above mentioned chemicals can be induced *by plant hormones* (BROWN et al., KRAUS et al., SOLACOLU et al., etc.) and that the growth of *phytocarcinomata* can be *stimulated* by the *plant hormones,* vitamine C (HAVAS), as well as by certain *animal hormones* (HAVAS, STAPP). The attempts to bring *experimental proofs* of an analogous "hormonic" mechanism of action of colchicine have furnished the following preliminary indications:

(a) *tumour resolution* similar to that produced by colchicine had been obtained by interventions involving only *hormonal changes* in the plants;

(b) production of *adventive roots* on the stem of *Impatiens balsamina;*

(c) stimulation of wound *callus* formation (cherry laurel);

(d) stimulation of *flowering* (tomato, syringa, Forsythia, Prunus);

(e) obtention of epinastic and *hyponastic responses* (tomato, Tradescantia);

(f) inhibition or stimulation of some of the above colchicine induced growth effects through the *synergetic or antagonistic action* of certain *plant hormones* simultaneously administered.

L. HAVAS: Nature, vol. 140, p. 191, July 31, 1937.

———— *Bull. Ass. franç. pr l'Etude du Cancer,* T. 26, No. 6, December, 1937.

———— *Growth,* Vol. 2, No. 3, p. 257, October 11, 1938.

————*Nature,* Vol. 143, p. 789, May 13, 1939.

———— ———— *Chem. Prod.,* Vol. 2, No. 3, p. 61, July, 1939.

VII. *May, 1939.*

Reversely in view of the already mentioned analogies of action of colchicine in both animals and plants (I, II, V) and considering the above "hormonic" effects of the alkaloid in these last, the induction of similar *hormone-like actions of colchicine* was tried *in animals* and the following results were obtained:

(a) the *sexually induced color change* ("nuptial colour") of the fish *Rhodeus was obtained through the administration of Colchicine* in the same lapse of time (20 hours) as through that of testis hormone. Moreover, the simultaneous administration of both colchicine and the testis hormone produced an equally intensive effect in 1 hour 35 minutes.

L. HAVAS: *Nature,* Vol. 43, p. 809, May 13, 1939; *Arch. Int. Med. Expér.,* December, 1939 (in press).

(b) Some "hormonic" actions of colchicine alone, and in combination with thyroxine and thymus on the early development of the frog were ascertained: L. HAVAS and J. MEHES (in press).

(c) Some hormon-like actions of colchicine administered alone or applied with *plant hormones* were observed in the frog, and certain antagonistic effects of these hormones to colchicine were found in mice:

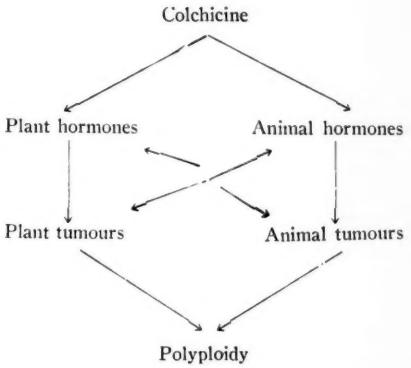
L. HAVAS: *Arch. int. Med. Expér.,* December, 1939 (in press).

I am of course fully aware that, until entirely confirmed, the parallel experiments and findings outlined above will have to be multiplied and given a much wider extension than it was my fortune to do. Meanwhile, as I can find no more plausible explanation for the manifold actions of colchicine as the one previously suggested, may I quote: "The 'hormonal' mechanism of action of the alkaloid can be exercised: 1—through the mobilization of the hormones already present in the organism; 2—through the deviation of the polarity of their translocation, causing a general or localized 'hyper' or 'hypoauxonomy'; 3—through the general or localized inactivation of the endogenous hormones; 4—through the synergism or antagonism of its action with the endogenous or administered hormones." (HAVAS: *Chem. Prod.*)

But, although this interpretation does not yet imply that colchicine *is* a hormone it might be asked in what connection with polyploidy these hormonal effects can be brought?

In answer it will be sufficient to recall that *polyploidy* has been frequently observed *in both animal and plant tumours,* which, as we have shown can be induced, stimulated or inhibited by both animal and plant hormones—and by colchicine.

The suggested essential interrelations can be perhaps best visualized as follows:



And so we have to come back to our starting point!

In concluding may I however repeat, in order to avoid misinterpretation that:

1. I did not wish to suggest that the American plant geneticists were acquainted with the above European pioneer work *at the time* when the effects of colchicine were tried for the first time in plants in the United States.

2. That the work of your countrymen, to commence with that of Professor Blakeslee, has my fullest admiration and that the question of priority becomes secondary in comparison with the intrinsic value of the results which they have obtained. It would be also ungrateful of my part not to acknowledge what my own work owes to their researches.

The real object of this letter is in consequence rather than criticism, *an appeal for collaboration*—geneticists and biologists need each other.

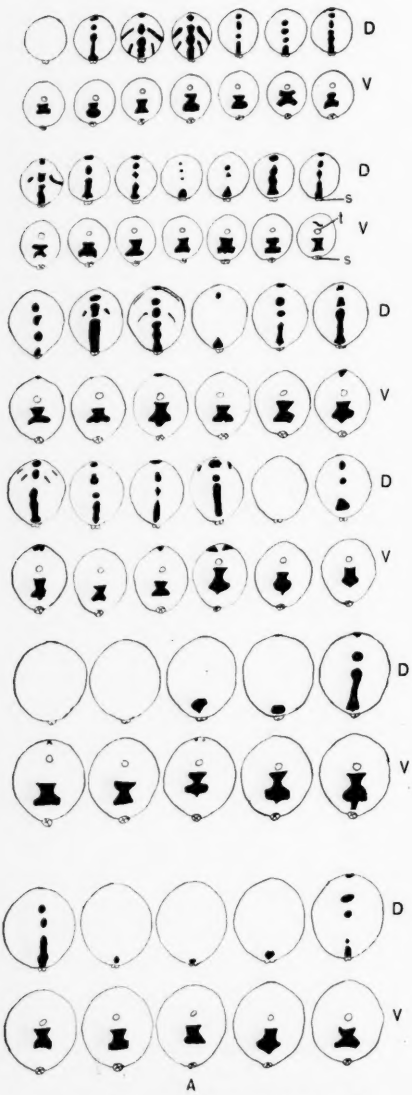
I hope that this appeal will not fall on barren ground in a country which can boast of students of *growth* of such universal culture as Hammett, of synthetically minded *cancerologists* and *plant pathologists* like Levine, Reimann, Andervondt, Riker, Brown, etc., of animal and plant *hormonologists* of such universal fame as Allen, Thimann, Went, Loehwing, the workers of Boyce Thompson Institute, and so many others, and moreover of an excellent school of *physiological genetics* where, as represented by such workers as Beadle, Ephrussi, Little, etc., all these sciences are cultivated.

Our poor continent, torn by war, is in sad need of such a collaboration.

Faithfully yours,

LASZLO J. HAVAS.

Brussels, Nov. 25, 1939.



BLACK WIDOW MARKINGS

Figure 7

A fifth of the largest spiders had no marks on the back of the abdomen (*D*) while only a thirteenth of the smaller spiders were unspotted. All had some variation of the "hour-glass" spot on the under side of the abdomen. *A* shows the most similar pair. The markings shown are in red, on a black background.

VARIATIONS IN THE MARKINGS OF THE BLACK WIDOW SPIDER

WHILE carrying on some research upon the black widow spider I noticed, as others have, of course, done, the variability not only in size of the animals but in the numbers, color and and arrangement of the spots on the abdomen. The variation in size was no more than may be expected in any form; the variation in abdominal marking was so great as to be worthy of note, especially in an animal that is as notorious as the black widow is at the present time.

In no individuals studied were any markings seen on the jet-black cephalothorax.

The well-known hour-glass spot on the ventral side of the abdomen was present in all specimens examined, but there was wide variation in the size and shape of the spot, from the typical, symmetrical hour-glass to an outline bearing little resemblance to that ancient timepiece. There were, occassionally, other small red spots on the ventral side of the abdomen, anterior to the hour-glass spot.

The dorsal abdomen varied from a uniform black to a black with as many as eight red spots of all sizes and shapes. It will be noticed that two of the ten largest spiders figured have the dorsal abdomen without spots, while of the medium and smallest spiders only two out of 26 have unspotted dorsal abdomina.

Of these 36 spiders, selected at random, only two were so similarly marked as to appear practically identical. These two are shown in the outline marked *A*.

The projection, *S*, on all of the outlines indicates the position of the spinnerets; *T*, the point of attachment of the cephalothorax; *D* and *V* mean dorsal and ventral aspects. All outlines are about twice natural size.

A. M. REESE

West Virginia University

MENTAL AND PHYSICAL TRAITS OF IDENTICAL TWINS REARED APART

Case XX. Twins Lois and Louise

IVA C. GARDNER AND H. H. NEWMAN
Baylor University and the University of Chicago

SINCE the publication of the book *Twins: A Study of Heredity and Environment,* in which nineteen pairs of identical twins reared apart were fully described and analyzed, only two further cases have come to our attention. This was a surprise, as the authors confidently expected that the publication of their book would help to bring to light many new cases. The first of the two new cases consists of a pair of young men in Pennsylvania. One of these twins, with whom we have had considerable correspondence, is interested in our problem and anxious to have their case studied, but the brother is, we fear, somewhat of a hoodlum and refuses to submit to examination. This is unfortunate in view of the fact that the two brothers now seem to be so different in their personality traits.

The second of the new cases is the one here described and listed as Case XX in our series. This case really belongs to Dr. Gardner, who discovered it, but she has been kind enough to have Lois and Louise studied by our methods and to let us count them as rounding out our twenty cases. Dr. Gardner, Professor of Psychology at Baylor University, Waco, Texas, gave the psychological tests. Dr. Newman visited the twins in Waco, made the physical comparisons, took palm and finger prints, and attained a certain amount of personal familiarity with the behavior of the twins. The tests were scored by assistants of Dr. F. N. Freeman under the direction of Dr. K. J. Holzinger. The case was written up by Dr. Newman, with the concurrence of Dr. Gardner.

It might quite fairly be asked whether there is any point in adding another case of this sort to those already described. The answer to this query is that cases of this sort are very rare and difficult to locate. On this account we have been strongly urged to find and report all new cases discovered. The fact that our continued efforts have so far succeeded in securing for study only one additional case since Case XIX was studied in 1934, seems to indicate that we have nearly exhausted the supply of available cases in our present population. In another ten years or so there may be a new crop, but this crop will probably have to be harvested by a younger investigator. It seems surprising, considering the importance of such cases, that only one other case of identical twins separated in infancy and reared apart has been reported, namely the well known case reported by H. J. Muller in 1925. The few cases described by Hirsch and by Saudek consisted of pairs that were separated much too late in life and for only relatively short periods. Such cases are no better for a study of the heredity-environment problem than would be those numerous cases of identical twins who separate in youth or adult life as the result of going to different colleges, getting jobs in different places or marrying persons living in different parts of the country. The study of such pairs after a period of separation would doubtless yield interesting results, but the fact that the twins lived together so long would render them unsuitable for attempts to isolate hereditary from environmental factors.

The present case then may be the last that I shall report, for my colleague in twin research, Dr. Freeman, has gone to California and I shall soon be retired from active service in a university.

TWENTIETH PAIR REARED APART

Figure 8

Louise (left) and Lois were separated eight days after birth and except for brief visits had no contact until they entered Baylor University at eighteen. Physically they are among the most strikingly similar pairs encountered in a study of hundreds of pairs. Louise was educated in a rural school; Lois went to city schools. Louise ranked slightly higher in achievement tests than her twin, but the similarities were much more striking than the small differences.

Life Histories of Lois and Louise

The following data were collected by Dr. Gardner. These twins were born in a hospital in Temple, Texas, July 11, 1920. The mother died shortly after the birth and the babies were separated eight days after they were born. They have remained separate except for a few short visits until they were eighteen years of age, when they both entered Baylor University as freshmen. They had been together only a short time before they received some of the intelligence tests.

On the death of the mother Lois was taken by a friend of the family, but was never formally adopted. In consequence of this circumstance Lois has always used her family surname, which is different from that of her foster parents. In addition to this, there has always been a certain feeling of uncertainty and insecurity about her status. This is regarded by Dr. Gardner as perhaps the most important differential factor in the social environments of the two girls. Lois' foster father was and still is a certified public accountant and for one reason or another has moved about from place to place. He has, however, lived for about the last ten years in Waco, a city of some 60,000 inhabitants, where Baylor University is located.

Louise was adopted by her father's brother. Hence her surname has always been the same as that of her foster parents. There has been no question as to the permanency of her status. She has lived continuously since adoption in McGregor, Texas, a small town of some 2,000 to 3,000 inhabitants. Her foster father is a merchant and a leading citizen in this town.

Educational Careers

There has been very little difference of a tangible sort in the education of the two girls. Lois has had almost all her schooling in Waco, where there is a first-rate school system. Since she did not settle down in one place of residence until after she was eight years old, she may have had a belated start in her formal schooling. She was also transferred twice from one elementary school to another because the family moved from one district of the city to another. Thus Lois' primary school education was somewhat disturbed at the start and interrupted during its course. However, Lois lost no time from school.

Louise had her entire primary and secondary education in the McGregor school, which has few teachers and few elective subjects. It might be assumed, though without adequate evidence, that this small rural school was inferior to the large city schools attended by Lois. If so, Lois should be better educated than Louise. Later we shall see that this is not the case.

In addition to their academic schooling, both have had the advantage of some artistic training. Lois had a special course in speech and private lessons in drawing and painting, in which she showed talent above the average. Louise has had two years of musical training. If we were to attempt to rate the differences in educational environment in grades from one to ten, as was done for our previous 19 cases, we would have to rate them as not more than half a unit different.

Social Environments

Lois has lived mainly in fairly large cities, Louise in one small frontier town in the cattle country. The contrast is one of urban life as over against a life almost rural in character. Dr. Gardner, who is acquainted with both homes, is inclined to believe that the cultural status of both is approximately the same. Economically, Louise' parents are perhaps in a position to give her more of the luxuries of life than Lois has had, but the difference is slight. Lois had a foster sister and a foster brother, respectively 10 and 13 years older than herself. The foster sister still lives at home and is very fond of Lois. Louise also had a foster sister and a foster brother about the same ages as those of Lois, but both married and left home before Louise was ten years old. Louise has therefore been an only child in the family nearly half of her life.

As was said earlier, perhaps the most important social environmental difference between the two girls inheres in the fact that the status of Louise has always been secure, while that of Lois has been uncertain. There was at one time some talk about Lois going back to live with her own father who has remarried, and also there was for some time a discussion as to whether Lois would not be better off if she lived with Louise in the latter's home. Lois is especially fond of her foster mother and has never wanted to be taken away from her. The possibility of being sent away from her present home has always been a disturbing factor in her otherwise happy life.

Physical Resemblances and Differences

At the present time these twins are as much alike as the proverbial two peas. When the junior author interviewed and compared them he never learned to distinguish one from the other. When a question was asked, they almost invariably answered in unison, using almost the same words in the same tone of voice. Our judgment is that the twins rank among the most strikingly similar pairs of identicals we have ever seen, and we have seen hundreds of them.

We are informed that at birth Lois weighed six and Louise five pounds. At present (March, 1939) Louise, the smaller at birth, varies from five to eight pounds heavier than Lois. Lois at six years of age had a severe attack of scarlet fever from which she nearly died. Louise never has had scarlet fever, but has had whooping cough and mumps, which Lois has escaped. Both have had measles and chicken-pox. Louise, when 12 years old, had an abscess on the spine, which was nearly fatal. Neither girl is robust, both rather tall and slender, and both have frequent colds. Both are attractive, good looking and vivacious.

A careful physical comparison showed that they are exactly the same in height, 5 feet, 4¾ inches. Louise weighs 105 to 108 pounds, while Lois used to weigh 100 pounds all the time. Dr. Gardner notes that Lois has gained this summer and now weighs as much as Louise. Both are strongly right-handed. The hair whorl of both is indeterminate, there being no distinct crown. The hair color, form and texture are the same in both, very dark brown, almost black, soft and straight. Both have scattering white hairs. The complexion of both is medium dark and clear. Eye-color is the same in both — light brown, with a narrow gray rim flecked with red. The four ears are indistinguishable, small, closely set and with small attached lobes. The hands are remarkable in both for the long, narrow palms, long, slender fingers, and the little finger proportionately very short. The finger nails of Louise are normal and well kept, but those of Lois were in poor condition because of her habit of biting them. Dr. Gardner reports that Lois has recently almost cured herself of nail biting and takes pride in the improved appearance of her nails. The teeth are extremely similar in individual size, shape, color and condition, but Louise's upper incisors overlap a little more than those of Lois. Both have two or three small fillings in molar teeth but in general the teeth of both are in much better than average condition. The head length of both girls is 17 cm.; Louise's head width is 13.75, that of Lois 14 cm. It seems fair to say that at the time we first examined the twins, Louise was physically slightly superior to Lois, as was indicated by her more nearly normal weight. Was this a consequence of rural life, of having escaped scarlet fever, or of having suffered less nervous strain? It should be remembered that at birth Louise was a pound lighter in weight than Lois and that, as a rule, differences in birth weights of identical twins persist for a long time, sometimes for life. Dr. Gardner says that when the twins returned to college this fall they weighed the same and Lois seemed to be as strong as Louise.

Are These Twins Monozygotic?

As was stated earlier, these twins are among the most similar that we have ever examined. This would ordinarily

be sufficient grounds for a monozygotic diagnosis. Our usual method of diagnosis, however, involves using palm and finger prints as a check on the general similarity diagnosis.

The palm and finger prints of these twins, though somewhat less similar than those of the average of identical twins, are in all respects well within the limits usually set for a monozygotic diagnosis. The shapes of hands and fingers are unusual and highly similar, as is the general texture of the friction ridges.

Palm-print formulae.

Left hands.
Louise: 9.7.5." 1 – tt′ – A^u/A^c. O.O.O.V.
Lois: 11.X.7. 1 – tt′ – A^u/A^c. O.O.O.V.
Right hands.
Louise: II.X.7.5.′ – tt′ – A^u/A^c. O.O.L.O.
Lois: 11.X.7.5.′ – tt′ – A^u/A^c. O.O.L.d.

The two right hands are extremely similar and are decidedly more alike than either right hand is to own left. Louise's left hand is considerably different from the other three hands. This three-alike-and-one-different situation is characteristic of many pairs of identical twins.

Finger-print formulae.

	Left	Right
Louise:	A^u. W. W^{lr}. W. W^{lu}.	U. W^u. U. W. U.
Lois:	U. W. W. W. W^{lu}.	U. W^u. U. W. W^{lu}.

The homolateral correspondence is 7½, which is just above the minimum requirement for monozygotic diagnosis according to the Stocks' test.

Ridge counts of the finger patterns, counting all ridges, is as follows:

	Left	Right	Total
Louise:	72	67	139
Lois:	75	69	144

There is a difference of only five ridges between these totals, which is very low even for identical twins.

Using MacArthur's test for identical twin diagnosis, we find that homolateral differences for the four items (ridge counts, palm lines, palm patterns, and finger patterns) amount to 20.9 per cent, while left-right differences are 37.1 per cent. Since the average homolateral differences for identical twins is 18.9 per cent and that for fraternal twins is 37.9 per cent, we see that this pair of twins is so close to the identical-twin standard and so far from that of fraternal twins that even MacArthur would never hesitate to diagnose them as identical twins on the basis of their palm and finger prints alone.

Tests of Ability

On the *Stanford-Binet* Test Louise was rated 18 years, 1 month in mental age, and Lois 17 years, 1 month. Using the 16 year standard, these M.A. grades were transmuted to I.Q. grades, giving Louise an I.Q. of 110 and Lois 107, a difference of three points, which is less than the average difference of 50 pairs of identical twins reared together.

On the *Otis Self-administering Test* of mental ability Louise had a score of 54 and Lois one of 53. The difference is not significant.

On the *International Group Mental Test* Louise had a grand total of 224 points, Lois 215 points, a very small difference, but again in the same direction as the other two ability tests. These tests are consistent in showing that Louise is slightly superior in mental ability.

Achievement and Scholastic Aptitude Tests

On the *Stanford Achievement Test* Lois was given an educational age of 16 years 11 months, and Louise of 16 years 8 months, a difference of 3 months in favor of Lois, who was somewhat inferior in the mental ability tests. Lois was distinctly superior in arithmetical computation, having a score of 148 as compared with 108 for Louise. In all other items of the test Louise was equal or slightly superior. Doubtless the great difference in arithmetic ability is a reflection of differences in teachers.

On the *American Council Test* for college freshman, which was given to the twins when they first entered college in the autumn of 1938, Louise had a score of 99, a percentile of the Baylor group of 82; while Lois had a score of 86, a percentile of 69. This difference of 13 points in percentile rating is regarded by Dr. Gardner as significant and supports the judgment resulting from the

ability tests that Louise is somewhat superior in ability.

On the two achievement tests we find that one twin is superior in one test and the other twin is superior in the other, but the superiority of Louise, who performed better on the ability test, is a little more marked and more consistent.

On the whole, the mental tests are fairly consistent in showing that Louise is a little superior to Lois, the one exception being in arithmetic. As a matter of fact, Lois, the foster child of an accountant, might have been expected to have been better drilled in arithmetic than Louise, the foster child of a merchant. It should be emphasized, however, that these twins are no more different in mental traits than the average of identical twins reared together.

Personality Tests

On the *Downey Will-Temperament Test* the item, Resistance to Opposition, was inadvertently missed for both girls. On the remaining items Louise scored 56, and Lois 58, a very close correspondence. In addition to this, the correspondence with respect to different test items was striking. Both were high in the same five items, both very low in the same two items (motor inhibition and vocational perseveration), and both were about average on the others. The *Will-Temperament* profiles are very strikingly similar in appearance — as similar as in any of the nineteen previous cases of separated twins. From this we may judge that Lois and Louise are closely similar in the types of personality traits measured by the Downey Tests.

On the *Pressy X-O Test* the differences are somewhat greater as shown in Table I. This table shows that Louise had a considerably higher score on words crossed out in the three tests, but a lower score on unpleasant words. Lois, however, had a higher score in deviations, which means a greater departure from the average reaction. Louise, the twin who is supposed to have always had a greater feeling of security, has considerably more worries than Lois, who has a feeling of insecurity. This is an unsuspected and contradictory result. Dr. Gardner comments that Lois had one "big worry"—a fear that she would be taken from her foster mother—while Louise worried about a great many things, some of which are akin to superstition. In general, if higher scores on this test indicate a more neurotic temperament, Louise is the more neurotic, whereas other indications would tend to the opposite conclusion.

On the *Woodworth-Mathews Personal Data Sheet* the twins answered 69 out of 75 items alike, and on the six items in which the answers differed Louise expresses various types of fears in five replies and Lois admits to having very strong superstitions in item 73. On the whole, though the correspondence is unusually close, Louise is a little more neurotic.

On the *Kent-Rosanoff Free Association Test* the girls made exactly the same response on 19 items and essentially the same on four more. Lois made 19 distinctly unusual responses and Louise only nine. On the whole, the average commonness of Louise's responses is distinctly greater than that of Lois. While this test indicates very close resemblance in emotional status, it is clear that Lois has more unusual associations and therefore is somewhat more neurotic. The results of this test are opposed to those of the Woodworth-Mathews Test, in which Louise was more neurotic. They tend to cancel each other. As Dr. Gardner gave this test, she noticed the twins' reactions.

TABLE I. Differences on the *Pressey X-O Test*

Test	Words Crossed Out		Deviations	
Test I	Louise	Lois	Louise	Lois
Unpleasant words	13	17	7	7
Test II				
Words associated in mind with given word	43	28	10	12
Test III				
Things thought wrong	79	65	14	14
Test IV				
Worries	52	43	14	14
Total	187	153	40	46

They were giving words that came to them from the stimulus word and both seemed to think it funny. It is often thought that one may unearth complexes in such a manner. She gave the tests separately. The girls reacted very much alike, as they laughingly replied to the stimulus word. Dr. Gardner does not think there is much reliability in the free association test as a test of neurotic tendency, although it does help locate complexes.

An additional personality test, the *Bernreuter test*, not given to most of the pairs of separated twins, was given by Dr. Gardner to Lois and Louise. Without going into any description of this test, scores of the six sections are given below.*

Both girls are high in neurotic tendency, extremely low in self-sufficiency, fairly high in introversion, very low in dominance, high in self-confidence, and very low in independence or non-sociability. In all the items the two girls are strikingly similar. Dr. Gardner comments that this is a much better characterization of the girls that that derived from previous personality tests.

With the exception of the fact that Louise writes a somewhat larger hand, the handwriting of the two girls is more similar than is usually the case even in identical twins reared together and taught to write in the same school. This might be regarded as additional evidence that these twins are extremely similar in certain personality traits.

Summary of Personality Tests

All of the tests are consistent in showing that these girls have highly similar personalities, that Louise, the twin who has had less reason to worry or to show neurotic symptoms, worries a little more and is a little more neurotic than Lois, who seems to think she has always been rather nervous and uncertain about her status. It must be admitted, however, that the neurotic symptoms are no greater than those found in average persons and that the differences here are slight and may not be significant.

Observations of the overt behavior of the two twins tend partly to confirm the results of the tests. Lois meets the public somewhat better than Louise, possibly because she lives and goes to college in her home city. For the same reason it appears that Louise, who is more or less the stranger, is somewhat more timid than Lois. This is in agreement with the formal tests. In contrast to this is the fact that Lois, until recently, has exhibited nervousness by biting her nails, which Louise never does. It is difficult to assess the significance of this piece of nervous behavior.

On the whole, it is our considered judgment that the resemblances in personality traits between these twin girls far outweigh their differences. In the only personality test in which a comparison is possible with identical twins reared together (the Woodworth-Mathews Test) these girls are much more similar than any but a very few pairs of identical twins reared together.

Summary

The one most noteworthy fact about these twins is that in all three respects —physical, mental, and temperamental —they are the most similar of the twenty cases of separated identical twins so far studied. Not only this, but they are more similar in all these respects than the majority of pairs of identical twins reared together. It might be argued from this that the environments of the two girls, Louise and Lois, have been as similar as that of a pair of twins reared together, but such an argument loses force when one considers the actual environmental differences known to have existed. It might still be argued that, although the environments of the two girls differed in many respects, the favorable and unfavorable factors in

*Bernreuter Test results:

	B1-N	B2-S	B3-I	B4-D	F1-C	F2-S
Lois	+107(96%)	−136(1%)	+44(86%)	−68(6%)	+162(97%)	−114(6%)
Louise	+106(96%)	−130(1%)	+46(87%)	−75(5%)	+164(97%)	−104(10%)

the two environments tended to equal each other and to give the same result as though they had been entirely identical. It would be difficult either to affirm or disprove such a contention.

The results of this study would seem to us to indicate that what differences have existed between the environments of the two girls have not been sufficiently great to have effected any very tangible differences in them, physically, mentally, or temperamentally. It appears from the previous cases, however, that where environmental differences are rather large fairly large differences result. Possibly, and even probably, we are dealing here with a matter of thresholds of influence and that, unless a certain threshold of difference in environment is reached, the organisms do not respond differentially. There are possibly different thresholds for the various and sundry environmental variables and the threshold of effectiveness for some may be more easily reached than that for others.

A fairly common criticism of our book on twins is that it plays up the environmental factors and tends to minimize the hereditary factors. This criticism is probably justified to some extent. The main reason for whatever overemphasis on environmental factors exists in the book is that two of the three authors are specialists in education and tend to be environmentalists at heart. It seemed also to all of us that most twin studies prior to our own had not given sufficient attention to the environment as a modifying factor. Hence in our work we may have gone too far in the opposite direction. Dr. Gardner comments that in our Case XI there was an educational difference of at least 13 years of schooling, accompanied by a difference of 24 points in I.Q. The twin with only third grade schooling rates low normal while the one with the college education rates high normal. Here we have a case in which hereditary endowment has been greatly limited in its expression by adverse environment.

The geneticist of the group of authors must confess, however, that throughout the whole study of identical twins reared apart, he was much more impressed with the very great intrapair similarities of these twins, after they had been exposed to all sorts of environmental differences, than he was with their differences. In only four out of the twenty pairs of these separated twins were there any very striking differences, exceeding those of some identical twins reared together. The remaining sixteen cases show no greater mean differences than did the fifty pairs of identical twins reared together. It might be concluded then that in these sixteen cases, although we know of many differences in their environments, these differences were not sufficintly large to reach the threshold of effectiveness. This may seem to be a strong argument for the hereditarian side of the controversy. Nevertheless it would be a mistake to go too far in this direction, for if one thing is clear in our results it is that fairly large environmental differences do modify physical, mental and temperamental traits and produce proportionately large differences even between hereditarily identical individuals.

THE GENETICS OF COAT COLOR IN HORSES

W. E. CASTLE

IN a recent number of this JOURNAL, Gremmel[1] has made an important contribution to our knowledge of the structural arrangement, chemical nature, and genesis of the hair pigments of horses. In one point only do his observations seemingly contradict those of most other students of horse genetics, in that he insists that only a single kind of pigment is found in the hairs of all color varieties. This is at variance with the findings of others who recognize a category of chocolate pigmented animals, the chocolate character being recessive in crosses with all black pigmented varieties, exactly as in rodents and dogs. But the important chemical studies made by Gremmel with colorimeter tests, indicate clearly that pigments from chestnut, blood bay and mahogany bay horses belong together in one category, and black, bay, and seal brown in another *dissimilar* category. This finding agrees fully with the findings of Crew and Smith[2] who report the first (brown pigmented) group to be recessive to the second (black pigmented) group of colors. We may accordingly withhold assent to Gremmel's formal conclusion as to a single kind of pigment, since it apparently is at variance with his own careful chemical studies, as well as with the genetic interpretations of others based on a study of pedigrees recorded in stud books.

Gremmel offers a tentative genetic analysis of the fundamental color types, involving three major genes, *A*, *B* and *C*, each of which exists in dominant as well as recessive alleles and in all possible combinations. He does not assign a definite and specific function to each of the three genes, so it is difficult to compare their action with that of color genes in other mammals. Yet a comparative study of the pigments of mammals and of their genesis indicates that the process of pigment formation is similar in all and the end products comparable.

I am tempted therefore to offer an alternative analysis based on a comparison with other mammals and assuming the existence of four rather than three principal genes. These are (1) a wild pattern gene, comparable with the agouti of rodents and wolves, having at least three alleles, A = dun, A^b = bay, a = no wild pattern; (2) a black-brown pair of alleles, B = black, b = brown; (3) a color gene C, which has one or more recessive albinistic alleles (C^y, c?); and (4) an extension gene E, having a dominant mutant allele, E^D, which obscures the wild pattern in the presence of A or A^b, like the dominant black of rabbits seen in Punnett's "agouti-blacks."

On this basis the principal genotypes would be as follows:

Dun = $A\,B\,C\,E$, sooty yellow with dorsal stripe, black mane and tail, sometimes with bars on legs, original wild type.
Bay = $A^b\,B\,C\,E$, red with black mane and tail.
Black 1 = $a\,B\,C\,E$, uniform black (recessive).
Black 2 = $A\,B\,C\,E^D$ or $A^b\,B\,C\,E^D$, black with light muzzle, "seal brown," "black dun," "mouse," may produce bay or dun in crosses with Black 1.
Brown 1 = $a\,b\,C\,E$, chestnut or liver, uniform brown with brown mane and tail.
Brown 2 = $a\,b\,C\,E^D$ or $A^b\,b\,C\,E^D$ or $A\,b\,c\,E^D$, chestnut, "blood bay," "mahogany bay," brown mane and tail.
Sorrel 1 = $A^b\,b\,C\,E$, golden red, mane and tail red or brownish.
Sorrel 2 = $A\,b\,C\,E$, light red, mane and tail red or brownish.
Ysabella or albinistic = A (or A^b or a) $b\,C^y$ (or c) E (or E^D), "blanco," Palomino, "cream," "albino." Coat colors range from golden red to almost pure white with faintly pigmented eyes, always with light mane and tail.

Heterozygotes between different "albino" alleles are probably phenotypically intermediate as in rodents.

Additional dominant pattern genes are:

Gray = *G*, which in combination with any of the foregoing genotypes produces white hairs interspersed with the colored ones in the *second* and all subsequent coats increasingly, but not in the first; also

Roan = *Ro*, which in combination with any of the foregoing genotypes, with or without *G*, produces white hairs interspersed with the colored ones in the *first* coat and in all subsequent coats increasingly with age.

Several types of white spotting (pied, calico, etc.) are produced by mutant genes, one of which is strongly dominant, others recessive.

This analysis, I think, involves fewer contradictions with the stud book records than any other with which I am acquainted. It involves only one feature not previously used in interpreting horse genetics, viz., the suggested existence of both dominant and recessive blacks, a well known occurrence among rabbits, and among dogs.

I am greatly indebted to Dr. G. W. Salisbury and Mr. J. W. Britton of Cornell University for access to their extensive data on Palomino horses, on which they will report elsewhere.

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Where Are the Thoroughbred "Palominos"?

To the Editor:

Fresh air from Arizona and Texas entering the old Coat-Colors-in-Horses castle? The allusion is to the work by Fred Gremmel published last October in your Journal. Daring—idol shattering even—as that new genetic hypothesis sounds, there is no gainsaying the chemical and histological facts Gremmel has so lucidly arranged, and for which the daring is not so daring after all. The Gremmel hypothesis, for instance, would make bay recessive to black, against the nearly unanimous results of the statistical studies with Stud Books. After his chemical and histological observations, Gremmel seems to be allowed to put the seal browns in the black class. As they are numerous, and appear mixed with bays in most of the Stud Books and corresponding studies, these studies are vitiated and their conclusions cannot be conclusive when it comes to the relation of dominance between bay and black. I am not saying this off-hand, but after perusal of the Sturtevant, Walther, Anderson and Winge statistics. It must be recognized, however, that the numbers are overwhelmingly against the possibility of black × black giving any bays.

There are, of course plenty of debatable points in what is, in Gremmel's words, only a "tentative hypothesis" about the basic colors. One point at least seems untenable and that is the interpretation of pseudo albino and Palomino. The breed of the race horse or Thoroughbred, by the long-closed character of the Stud Book, must be regarded as an isolated—and very numerous—population. It may be safely said that for the last fifty years, and probably for a much longer period, the two mentioned colors are unknown in the breed. On the other hand, all the other basic colors are frequently found, even the sorrels with flax mane and tail which Gremmel would make *AAbbcc*, when Palominos are *Aabbcc*. It would take an additional hypothesis—too elaborate a hypothesis, in fact—to explain that.

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Dr. Castle's analysis seems to answer Dr. Odriozola's objection—the C^{y}-*c* alleles being absent in the Thoroughbred. —Ed.

THE GENETICS CONGRESS

MICE and men reported from Edinburgh after the Congress (JOURNAL OF HEREDITY for September 1939) but since then there has been silence, as far as getting into the record any details of the Congress. On account of the disruption to trans-Atlantic travel caused by the declaration of war between England and Germany, September 3, the American delegation to the Congress was considerably delayed in getting back. Only two failed ultimately to return, Dr. and Mrs. F. W. Tinney of the Division of Farm Crops of the University of Wisconsin. They were among about a dozen members of the Congress who took passage on the ill-fated *Athenia*. Just how they were lost is uncertain, but there is reason to believe they may have been on the lifeboat which was struck by the propeller of one of the rescue ships.

Several other members of the American party were on the *City of Flint,* which left Scotland about the same time as the *Athenia* and which picked up some two hundred survivors of that disaster. The *City of Flint's* next voyage carried it to Murmansk and nearly to Germany, as well as to the front pages of the newspapers for many days.

After the Congress a number of the American members crossed to the Scandanavian countries and took ship from there, so that the last of them did not return for over a month after the Congress had adjourned.

It has not been easy to get those who did go to tell in detail just what happened at the Congress. No one person was able to cover all of it, and the tension under which it was conducted added a further mental hazard. Dr. R. A. Emerson, the official American delegate appointed at the 1932 Congress, has kindly given us his impressions. A statement by the Secretary-General, and later President, Dr. F. A. E. Crew is also quoted in part from *Nature,* for September 16, 1939. This details some of the difficulties and gives the formal actions taken at the Congress. A supplement to *Nature* (London) devoted to the Genetic Congress was published on November 11, 1939. This contains summaries of the activities of each of the eight sections into which the subject matter was divided. Most of these were prepared by the section Secretaries or recorders, so that a complete statement is available regarding practically all of the meetings of the Congress. Publication of the *Proceedings* was planned, but no recent announcement regarding this has been received. The President and General Secretary is in the British Army, and genetical work at the University of Edinburgh has largely been suspended.—ED.

IMPRESSIONS OF THE SEVENTH INTERNATIONAL CONGRESS OF GENETICS

R. A. EMERSON

THE Edinburgh Congress was planned and held under conditions of extreme difficulty. Professor F. A. E. Crew, general secretary of the Congress, and his collaborators in Scotland and the British Isles in general deserve unstinted commendation for the way they planned and carried on to the end when they could not know from one week to the next during the whole last year whether a congress could be held at all. Those of us upon whom rested the responsibility for the success or failure of the Ithaca Congress in 1932 felt that the depression, which hit us after our preliminary plans had been made, was an almost insurmountable difficulty. But, compared to the year-long uncertainty in the Europe of 1939, our difficulties were minor ones.

It is a pleasure, in this brief account, to commend heartily the statement made by Dr. Otto L. Mohr of Oslo, Norway in opening the first plenary session of the Congress. Dr. Mohr was chairman of the international committee elected at the Ithaca Congress. It was no easy task so to phrase an opening address as to facilitate the harmonious association of representatives of European countries already practically at each others' throats. The Ithaca Congress made no mistake in electing Dr. Mohr.

The Edinburgh Congress exhibited most of the good and a few of the bad features of such congresses. There were many papers of real importance. As one of the older members, I am bound to say that these were presented for the most part by the younger workers who had new and worthwhile things to report. Also, as is usual, there were papers that might well have been omitted. Every congress has that problem to contend with and solves it or fails to solve it as was done at Edinburgh and at Ithaca. If papers are not accepted their authors will not attend. If the success of a congress is measured in terms of the number of members who attend, and if the possibility of publishing the proceedings depends on the number of paid memberships, the problem is a real one. Now that the decision has been reached to make the publication of proceedings a wholly voluntary matter resting with the local council, some of these problems may, perhaps, be avoided. It might be well in future to admit to the program only such papers as present new and worthwhile results. This might well limit membership correspondingly, but if no volume of proceedings is to be published, the need of a large paid membership would be less. Noteworthy papers would undoubtedly be accepted for publication in regular channels, and it might well turn out that the suggested change in plan would encourage attendance of many who would prefer to find out what is new and important rather than to rehash their own old materials and ideas.

Many exhibits of compelling interest were available for study at the Edinburgh Congress. They were given adequate space in well lighted rooms. The committee in charge left little to be desired in the way of installation and care of the exhibits. The exhibits would, however, have been a more effective part of the program if provision could have been made for their demonstration at stated hours.

I still marvel at the ability of the local council of the Edinburgh Congress to plan and to carry to a successful conclusion such a gathering at such a time. At best such an undertaking involves a tremendous amount of work. Having to revise—in fact, to reorganize—the whole program at the last moment, as was necessary at Edinburgh, required work *plus* determination to bring the Congress to a successful conclusion no matter what the odds against it. And it *was* carried to a successful end!

Report of Congress Activities

On September 16, 1939, the following statement signed by Professor F. A. E. Crew appeared in *Nature*.

The chief qualifications demanded of those who undertake the organization of an international scientific congress in these days would seem to be an unwarrantable optimism and a complete disregard for current political events. At no time during the last two years has it ever been certain that the International Genetical Congress would meet, and now it is indeed surprising that it ever met.

It will be remembered that the Permanent International Committee appointed by the Sixth Congress at Ithaca in 1932 accepted the invitation of the Russian geneticists to hold the next Congress in Moscow in 1937. Difficulties arose, however, which made its postponement necessary. Late in 1937, the International Committee had reason to doubt that the Congress could be held in Moscow even in 1938, and found it necessary to seek invitations from other countries. The committee of the Genetical Society of Great Britain was informed that if we in this country were prepared to take over from the Russians, we should be rendering both the science and the International Committee a very great service. Being assured that it was quite impossible for the Congress to meet in Moscow in 1938, the Genetical Society of Great Britain offered, in

October 1937, to stage it in Great Britain in 1939. This offer was eagerly accepted.

The committee of the Genetical Society formed itself into an organizing committee and decided that the Congress should meet in Edinburgh. F. A. E. Crew was appointed general secretary. Since N. I. Vavilov would have been the president of this Congress had it been held in Moscow as was intended, and since we wished to indicate in the only manner available to us our great respect for him personally and our ardent desire for the active co-operation of the Russian geneticists in the affairs of the Congress, an invitation was sent to Vavilov asking him to continue in the presidency.

During 1938 and the first half of 1939, the programme of the Congress slowly took shape. At times the organizing committee had reason to think that its was a hopeless task; at others it was greatly encouraged by the eager co-operation extended to them by geneticists from all over the world. A programme built up of more than four hundred papers of quality was ultimately issued, whilst in London, Cambridge and Edinburgh preparations for the reception and entertainment of some four hundred foreign guests were completed. Yet all the while doubt lurked in the minds of all of us. It was obvious to all that the day was not far distant when the flickering lamps of science would be dimmed, and the only question that concerned us was when this day would dawn. Should we be permitted to carry this Congress to its successful conclusion before we were required to leave the forges of knowledge and use tools that we had made, for unworthy and damnable purposes?

On the morning of August 23 there gathered for the first meeting of the Congress some six hundred geneticists drawn from fifty-five different countries. There could be no grander measure of the devotion that men of science give to the science they serve than this. They met in days heavy with anxiety, to consolidate the ground won by them and their colleagues during the last seven years. The Congress had met, but it lacked a president, for, a fortnight previously, information was received that no one from the U.S.S.R. would be attending. This was a wound which at the time seemed mortal, for it had been hoped that not only would Russia supply a president but also that fifty or more papers of outstanding quality would be presented. However, the vitality of the Congress was such that it survived, and for two days into its rooms none of the loud murmurings of the outside world entered. Looking back on those days, they now seem to have been a dream of what the world itself might be were all men reasonable.

It is true that according to the records there were Americans, French, Germans, Poles, and all the rest of a great geographical and political variety, yet these were divided by no barrier of language or of prejudice. They formed one harmonious whole, moved by a common purpose, to advance a science, to serve mankind, to cultivate a fellowship. But the murmurings outside grew in intensity, and towards the end of the second day the air became disturbed, and everyone began to remember his private responsibilities and duties. The first group to be seriously affected was the German delegation (34 strong), which received advice that it would be desirable for them to think of returning home. The advice they resisted until it became quite certain that if they delayed longer they might encounter very serious difficulties in transportation; and so, most regretfully, and bearing with them the good wishes of their colleagues, they bade us farewell. At the same time, owing to mobilization of the Dutch army, the delegation from the Netherlands (17 strong) decided that it was desirable that they also should go. From that moment onwards the citizens of Edinburgh looked upon the Congress as a trustworthy barometer. Hourly inquiries as to whether or not the Italians, the French and the Poles were still with us were received, and Edinburgh was comforted when it could be told that the French and the Poles were remaining with us, and that the Italians had no intention of departing.

The programme, of course, was sadly crippled by this time, and for a while the organizing committee considered the question as to whether or not, in the circumstances, it would not be advisable to bring the Congress to an abrupt end. However, the Congress itself decided that it should continue. There was little else that we could do save to indicate, by continuing, that science was worth serving even in days like these, and that it, of all things, offered the most enduring good to all mankind. But the Congress was shortened, for Sunday was made a working day. A brave gesture this, for it will be remembered that Edinburgh is the capital of Scotland. As things turned out, this decision was more than justified, for the meetings continued to be very well attended right to the end, and daily the congressists sat down to mighty repasts. Looking back upon the sessions from this short distance, it would seem that not one of them was remarkable for the announcement of any startling discovery, but that they were immensely useful in that, during them, differences were bridged and new lines of advance were traced.

The organizing committee received repeated expressions of congratulation for the way in which the Congress affairs were conducted. Those who congratulated us seemed to be unaware of the obvious fact that the quality of a Congress is determined not so much by its organization as by the personalities of its adherents. On this occasion we were more than ordinarily fortunate. From a great many countries there came strong delegations bringing contributions of knowledge, of friendliness, of charm. It was they who made this congress successful. To the Germans, to the French, to the Italians and to the Poles we are indeed indebted. They came at great personal incon-

venience to our aid, and they have left behind them most happy memories. The coming of an American contingent, 130 strong, made the success of the Congress inevitable. But it is too much to expect that the welcome they received would compensate them for the distress that they endured when the Congress ended, for then they found themselves stranded in Great Britain with no immediate hope of return to their own. Already at least seven of them have been exposed to the gravest danger, for these were passengers on the *Athenia* and their fate at the moment of writing is not known. Some forty others have still to make the dangerous passage.

At the first business meeting of the Congress, F. A. E. Crew was elected to the presidency rendered vacant by N. I. Vavilov's absence. A Permanent International Committee consisting of C. L. Huskins (Canada), O. Winge (Denmark), Boris Ephrussi (France), von Wettstein (Germany), F. A. E. Crew (Great Britain), A. Ghigi (Italy), M. J. Sirks (Netherlands), Otto L. Mohr (Norway), G. Dahlberg (Sweden), A. Ernst (Switzerland), M. Demerec (United States of America), was appointed. This committee later elected F. A. E. Crew as its chairman.

At the second meeting of the Congress it was agreed that, in the circumstances, it was quite impossible at this particular time for the International Committee seriously to consider invitations and to decide as to the time and place of the next congress. It was intimated that invitations had been received from Italy and America. The International Committee was given authority to designate the time and place of the next congress at some later date.

Following a motion brought forward by the International Committee, the Congress decided that it should not be regarded as essential or necessary for future congresses to publish *Proceedings*. This decision was reached because it was recognized that, in the past, a number of invitations to meet therein had not been received from several countries on account of the expenses that are associated with the publication of the *Proceedings*. It was recognized that complete autonomy must be given to the actual organizing committee of a particular congress as to whether or not *Proceedings* of that congress should be published.

The Congress considered the setting up of an international committee which would devise a scheme whereby animal and plant stocks of genetical importance could be maintained in times of emergency. The International Committee was asked to consider the matter and set up a committee for this purpose.

During the course of the Congress a group meeting was arranged for the discussion of the statistical requirements of the student of genetics. At the end of this meeting the following resolutions were passed:

(1) That the teaching of statistics should be carried out in conjunction with the teaching of practical genetics.

(2) That elementary statistical methods, in particular tests of significance and their validity, should be included in the ordinary mathematical courses given at secondary schools.

(3) That the General Secretary of the Seventh International Congress of Genetics be requested to transmit copies of resolution 2 to any educational bodies which he considers may be able to assist in its fulfilment.

(4) That the genetical societies of countries represented at the Seventh International Congress of Genetics be requested to discuss the teaching of statistics in relation to genetics and to transmit their final resolutions to the next congress.

In connection with the Congress, a meeting of mouse geneticists was held on August 24. (The report of this committee has already been published in the JOURNAL—ED.).

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THE CHRONOLOGY OF HOPE WHEAT

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THROUGH the courtesy of E. S. McFadden, Associate Agronomist in the U. S. Department of Agriculture, the author has had his attention drawn to some prevailing misconceptions concerning the time of origin of Hope and H-44 wheat varieties. In my article[1] on Recent Progress in Plant Breeding, reference was made to the prediction in 1920 by Hayes, Parker and Kurtzweil[3] that the transfer of immunity to black stem rust from emmer to common wheat was possible. Follows the statement that it was not until 1930 that McFadden[4] succeeded in combining the rust resistance of Yaroslav emmer with commercial qualities of Marquis wheat with the resulting introduction of two new varieties, H-44 and Hope.

McFadden, in a letter to the author, points out that his 1930 paper shows

that "Hope wheat was in existence in essentially its final form at the time Hayes and his associates expressed their opinion. The cross from which Hope wheat originated was made in 1916, and the first true-breeding vulgare segregates were selected from the bulk hybrid material in July, 1920, whereas the article in which Hayes and associates made their predictions was not published until September of the same year. It is true that the original selections were later reselected for characters other than rust resistance, but it is quite certain that no improvement resulted from this reselecting."

"Furthermore," quoting from McFadden, "Hope wheat was in existence before there was any American literature on chromosome numbers in wheat species. At the time the cross was made in 1916 practically nothing was known about the difficulties involved in such a breeding project. . . . If anyone deserves special credit for suggesting the breeding project that resulted in the development of Hope wheat, it is the late M. A. Carlton who in 1901 wrote in Farmers' Bulletin No. 139 as follows:

> For the experiment stations and for private wheat breeders, emmer will prove to be very valuable for improving our ordinary wheats. By using it in crossing with common varieties the valuable qualities that may thus be secured are: (1) Resistance to fungous attacks, (2) drought resistance, (3) increased fertility of the head, (4) nonshattering, (5) stiffness of straw, and (6) increase of gluten content of the grain. Crosses with emmer are readily made, and the result is a great increase in general vigor and hardiness. The prepotency of emmer is shown by the fact that in crosses, even where a common wheat is the female parent, characteristics of emmer crop out in the offspring of the first generation.

"Hope and H-44 were both distributed in the spring wheat belts of the United States and Canada for use as parental material in the spring of 1925, but official publication of the existence of such varieties was not given until 1930. As a matter of fact, discovery of the inheritance of the Hope type of resistance to stem rust as a dominant character was announced by Clark and Ausemus[2] in 1928, which was two years before my official announcement of the Hope variety."

Plant breeders in general and cereal breeders in particular will, I am sure, be grateful to Mr. McFadden for his timely corrections in the chronology of Hope and H-44 wheats.

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5,000 YEARS OF THE ARAB HORSE

COMBINING the critical judgment of a scholar and the skill of a story teller of ancient days, the author* lays before us the story of the Arabian horse, a breed which for thousands of years has been the companion and servant of the desert soldier, and in other lands the inspiration of horsemen, sculptors, and poets. Beginning his story by telling of his trip through the desert in quest of breeding stock, the author gives us a delightful first-hand picture of the ancestral home of the Arab horse. All this reads like high adventure; thrills, spills, dangers, and finally success in the purchase of suitable stock.

Here Mr. Brown skillfully spreads before us his theory that four distinct types of horses developed from widely diverse parent stock. The Arabian is

*Brown, W. R., *The Horse of the Desert*. $27.50. The Derrydale Press, N. Y. 1935.

one of these four original types that existed before the dawn of history. The isolated and nomadic characteristics of the Bedouin, the constant and severe environment combined to make this species a more definite type than the horses of other nations.

In support of his theory the author goes into elaborate detail over the anatomy and physical peculiarities of the Arabian. The reviewer can readily imagine a reader, after finishing this account of this horse, feeling that all three other types could just as well be put into cans for dog food. From the records shown here the Arab is superior in every way. His ability to maintain himself under adverse conditions, his speed, virility, longevity, weight carrying prowess, and gait are unsurpassed. Every known breed has been improved by the admixture of Arabian blood.

The Bedouins are among the first practical geneticists. Their family and their horse pedigrees are among their most prized possessions; these are passed down from father to son by word of mouth. Pedigrees are never written unless specifically ordered, although the horses are branded with a needle in a way to indicate the length of pedigree. The Bedouins exercise great care in the breeding of their horses, often traveling great distances in order to secure a desired mating. They always try to breed within their own particular blood lines. Although allowed by law, the stud is never hired out by the Bedouin, and he does not hesitate to refuse the service of his stud to a mare he feels is inferior. Pure breeding within lines is so fanatically carried out that if a mare is bred to an out-blood stallion, she, her foal, and all her produce lose caste from then on.

Careful breeding of the Arab for hundreds of years has eliminated faults so completely, Mr. Brown maintains, that they may be inbred to the second generation with excellent results. Breeders of Thoroughbreds have found it unwise to inbreed closer than the fourth generation.

The Bedouins use only mares in raiding parties and in war, since they are quiet, whereas the stallions give away their presence by neighing. The importance of pedigrees and proper breeding is shown when a truce is called in a battle between nomadic groups, not to discuss peace, but to relate to each other the pedigrees of the horses that had been captured, thus insuring the proper breeding of the animals which they hope some day to recapture.

Pure bred stallions are never altered, although impure ones are, a lesson which might have some application in human genetics. The Bedouin has no paddocks to keep his stallions in and allows them to grow up with the mares. Stallions used to breed are kept heavily shackled to make accidental leaps next to impossible.

The author lists and compares several closely related breeds; maintaining, as a good "Arab" should, that perfection is in direct proportion to the amount of admixture of Arabian blood. The author has shown that in the Arabian, more than any other, we have a pure bred animal. From his observations it appears that when a breed is pure, no harm can come from close inbreeding. This is in line with modern concepts of inbreeding, which is simply a means to bring to light recessive defects, so that they may be eliminated. The preservation of pedigrees and strict adherence to them in breeding have produced this superior animal.

The Horse of the Desert is the most readable book of its kind that it has ever been my pleasure to encounter. True to the Oriental verbosity of the Arab, the reviewer could go on page after page; but enough! horsemen should read and enjoy it for themselves. They will deem it to be one of their most valued possessions.

S. O. Beebe.

Sandy Spring, Md.

INDIVIDUAL BLOOD DIFFERENCES IN CHICKENS

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A NUMBER of earlier workers and, more recently, Thomsen†, have studied the question of blood groups in chickens. Some experiments the present authors carried out some years ago, taken in connection with the results of Thomsen seem to contribute to our knowledge in this field.

The earlier work indicates that the blood differences of chickens are inherited, but in order to trace the genetic behavior of individual agglutinogens it will clearly be necessary to obtain sera containing only a single or at most a small known number of agglutinins. Sera prepared by iso-immunization of chickens contain usually an indeterminate number of agglutinins.

The authors, in order to obtain solutions of single agglutinins, adopted the following procedure. Chickens were immunized with the blood of other chickens, as described by previous workers, but in this case reciprocally, i.e., hen number 1 was injected with the erythrocytes of hen number 2, and hen number 2 received cells from hen number 1. From this it was definitely known that the serum from number 2, for example, whatever agglutinins it might contain, did not at any rate contain any of the immune agglutinins present in serum 1, since these latter were all produced in response to agglutinogens in the blood of hen 2, and it is well established that an animal will not produce antibodies to antigens already present in its own blood.

Each such serum was tested against a number of bloods taken from pedigreed chickens at the State Poultry Farm at Amherst, Mass. No blood was found which was not agglutinated, a fact in itself indicative of the presence of at least several antigenic factors, so that we did not by chance come across any of the same combinations of factors as those present in the original hens furnishing the sera. Then each serum was absorbed with blood taken at random, and the test repeated. This time some bloods were found which did not react with the absorbed serum. The serum was then further absorbed with one of the positive bloods, and retested. This procedure was continued until no blood could be found with which an absorbed serum could be further absorbed, and still leave agglutinin for other bloods. Washed erythrocytes were used throughout.

In theory, this process if carried out with a sufficiently large number of bloods would give finally a number of solutions, each containing a single agglutinin, and equal in number to the total number of different agglutinins produced by all our injected hens. In practice, limitations of amount of material and strength of serum, specially the latter, prevented the certain establishment that only one agglutinin remained, and did not allow the preparation of any large amount of absorbed solutions. However, a large number of tests were carried out with five different agglutinin solutions on 19 different families of chickens. It is thought that these five solutions repre-

*The authors wish to express their thanks to Dr. F. A. Hays, Research Professor in Poultry Investigations, Massachusetts State College, for his assistance and permission to take blood from the birds.

†THOMSEN, O.: *Zeitschr. f. Rassenphysiol.* 7:47, 1934, and *Hereditas*, 19:243, 1934.

sent probably the closest approach thus far to single agglutinin solutions for chicken blood.

In order not to make any assumption about the blood factors themselves, the agglutinin solutions were numbered, and each chicken whose erythrocytes reacted with a solution was given that number, plus others if necessary. So a bird reacting with solutions 1 and 4 would be designated as 1-4, and one reacting with none, as 0. This latter designation does not imply any analogy with the human type *O;* it simply means that no reaction was obtained with any of our five solutions, which can not have contained all the possible chicken agglutinins. The occurrence of this type is possibly some evidence that we succeeded in approximating single agglutinins in our absorbed solutions. The family relationships are shown in Table I.

TABLE I. Blood grouping reactions of different families of chickens.

Sire	Dam	Daughters
2–4	4	4, 4, 4, 2, 2
2–4	0	4
4	4	4
4	4	4, 4, 4
2–4	1–4	1–2–4, 1–2–4
1–2–4	2–3–4	1–4
4	1–2–4–5	5, 4,1–2–5
4	1–2	0, 1, 1–2–4, 1–4
4	4	0, 4, 4, 4, 4
4	0	4
1–2	1–2–4	*2–3–4*, 1–4, 4
1–2	4	1–2–4, 1–2–4
1–2	4	1–2–4
1–2	4	*1–2–4–5*
4	0	*1–2*
4	1–2–3	0, 2, 2, 1–2–3
4	1–2–4	4
1	0	1
1	4	1–4, 1–4, 4

The numbers signify positive reactions with respective agglutinin solutions. Sires often fathered more than one family, as indicataed by brackets. Italics indicate exceptions to rule mentioned in text.

It will be noted that the results support the idea that these individual blood differences are hereditary, and, on the whole the rule that a character never appears in the blood of a chicken unless present in the blood of at least one of its parents. However, there are three cases, shown in italics, which constitute exceptions to this rule. It is interesting that Thomsen, who exhausted a mixed serum with the blood of the parents, and then tested the offspring, though he found the latter generally negative, found positive results in two cases. He states, "Es wäre ja aber auch sehr wohl denkbar, dass es Antigene mit rezessivem Charakter gäbe. . . ." If this is indeed the explanation of his and our observed exceptions to the above rule, it will presumably be the first time an agglutinogen has been found to be recessive (unless A_2 is considered to be such, or *O* is considered to be an agglutinogen).

It would seem that more extended work of this sort, carried out by some one having pedigreed birds at hand, would give results of genetic interest.

A TRANSLOCATION IN A WILD POPULATION OF GRASSHOPPERS

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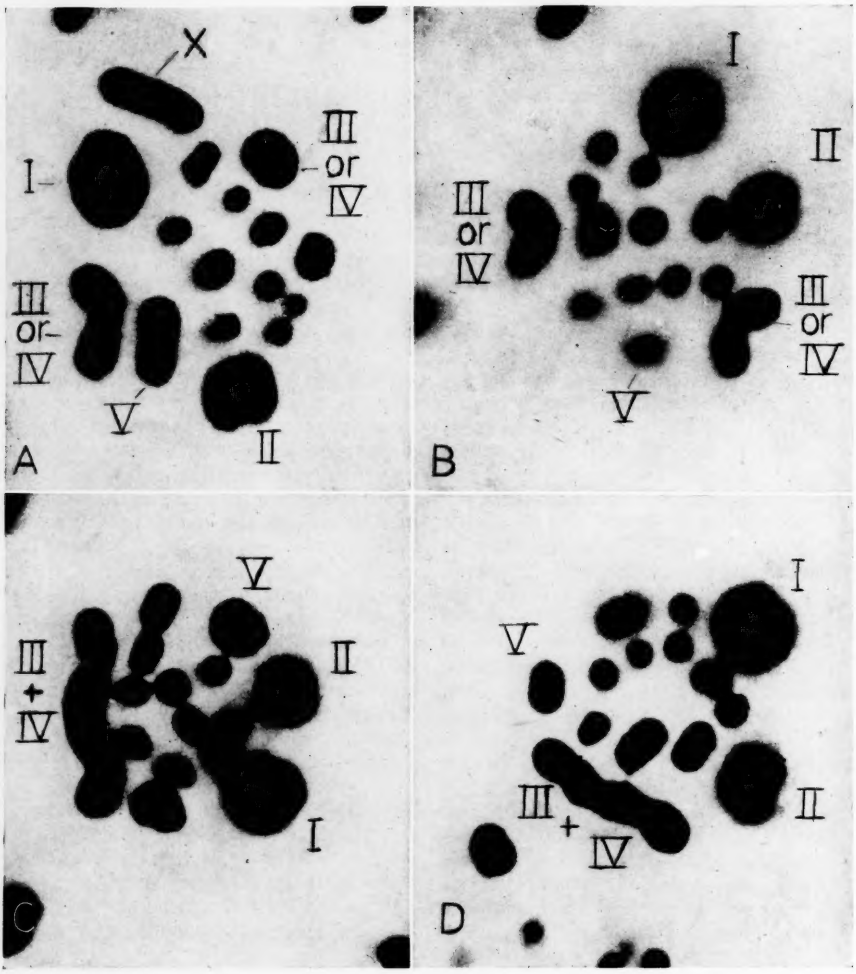
IN recent years it has been shown that many wild populations of animals contain sectional rearrangements of chromosomal material as well as mutant genes. That is to say, most wild populations which have been studied intensively have been found to be cytologically as well as genetically hetereogeneous. Not all the individuals in the population have the same genes, and even if they do possess an identical set of genes, these may be arranged in different sequences in different individuals. Thus Dubinin, Sokolov and Tiniakov[4] have found five different sectional rearrangements in 1666 chromosome sets of wild individuals of *Drosophila melanogaster*. All these were inversions of chromosomal regions not containing the spindle attachment (a type of inversion which Muller[6] has called *paracentric*). In other species of Drosophila the work of Dobzhansky and his collaborators (summarized by Dobzhansky[3]) has shown that the same situation occurs—that paracentric inversions are widespread, and are the only type of chromosomal rearrangement found. In another group of insects, namely the Orthoptera, Darlington[2] has likewise reported the existence of paracentric inversions in wild individuals. Here however, another type of chromosomal aberration was found, represented by the unequal bivalents seen at meiosis. It is not clear whether these are due to deficiency in one chromosome or duplication in the other.

None of the above mentioned workers have reported finding translocations in the populations they studied. This may appear surprising, since it is clear that translocations must have occurred in the course of evolution. The reason for the extreme rarity of translocations in wild populations appears to be the fact that whenever they are present in the heterozygous condition they give rise to a proportion of "unbalanced" gametes which lack certain chromosomal regions and have others present in duplicate. Individuals doubly heterozygous for a translocation will usually produce 50 per cent "unbalanced" gametes. Since the latter will usually be incapable of giving rise to viable zygotes it is clear that most animals heterozygous for a translocation will have their fertility rather severely reduced. On the other hand the fertility of individuals homozygous for the translocation will usually be normal. Thus on *a priori* grounds we should expect that translocations should only very rarely establish themselves in natural populations; but once established (through smallness of the population or some other cause) there will be no selection against the homozygotes.

In agreement with this *a priori* reasoning we find that a few, but only very few, cases of translocations in wild animals have been recorded. The only really clear case (apart from translocations adjacent to the spindle attachment which do not necessarily lead to a reduction in fertility when heterozygous) is that reported by Carothers.[1] A single male grasshopper of the species *Trimerotropis citrina* showed a ring or chain of four chromosomes at the first meiotic division. It appears that the translocation was of the "mutant" (and not the "insertional") type. Unfortunately since the condition was only observed in a single animal out of fifteen from the same locality, it is not clear whether it had established itself in the population. It may well have been an aberration in a single individual.

The present author has found a trans-

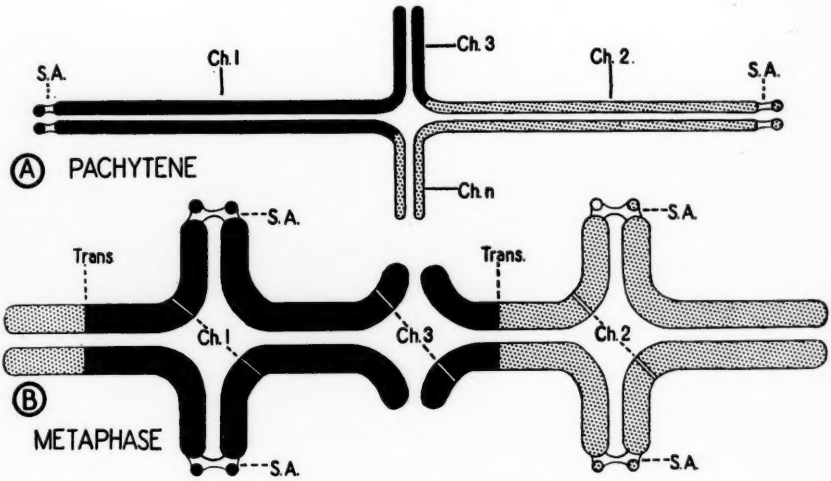


GRASSHOPPER CHROMOSOMES SHOWING TRANSLOCATION

Figure 9

Photomicrographs of metaphases of the first meiotic division in *Metrioptera brachytera*. *A* is from an individual apparently not heterozygous for the translocation, and shows fifteen bivalents and the *X*-chromosome. *B* is the same from an individual heterozygous for the translocation. This cell does not show the "translocation chiasma" between bivalents III and IV. *X*-chromosome out of focus. *C* and *D* show chains of four chromosomes (III-IV). *X*-chromosomes out of focus in both cells. All × about 4,000.

These photomicrographs were taken in the Department of Zoology, Columbia University, New York City, during the author's tenure of a Rockefeller Fellowship in the U. S. A. I am very grateful to Prof. Franz Schrader, who placed the facilities of his laboratory at my disposal during this time.



DIAGRAMS OF MUTUAL TRANSLOCATIONS

Figure 10

Diagrams of Pachytene and first Metaphase in an individual heterozygous for a mutual translocation. The chromosomes have quasi-terminal spindle-attachments (S.A.). Three chiasmata are formed (Ch. 1, 2 and 3). Ch. *n* indicates a position where a chiasma might theoretically occur, thus leading to a "ring of four."

location in a wild population of grasshoppers which appears to be similar in type to that found by Carothers. The species in this case is *Metrioptera brachyptera* L. (Tettigonidae) and the population inhabits a sphagnum bog in the New Forest (Hampshire, England). The bog has an area of about half a square mile. The first individual carrying the translocation was found in 1934 and some more animals bearing the same translocation were found among the material collected in 1937. It is thus clear that in this instance the translocation has become well established and has persisted for a number of generations.

Cytological Details of the Case

A general description of the chromosomes and meiosis of *Metrioptera brachyptera* has already been published by the author.[6] There are 31 chromosomes in the diploid set of the male, and 15 bivalents are formed at meiosis, the sixteenth chromosome being the *X* (Figure 9*A*). All the chromosomes are of the 'rod-shaped' type, with quasi-terminal spindle attachments. The bivalents will be referred to as I to XV in order of size, I being the largest. Chromosomes III and IV are unfortunately so nearly equal in size that they cannot be distinguished with certainty under the microscope. It is between these two chromosomes that the translocation has taken place. As in Carothers' case a chain of four chromosomes (instead of two bivalents) is sometimes found. This chain of four only occurs in about one per cent of the spermatocytes; it is, however, perfectly definite and recognizable wherever it occurs (Figure 9*C* and *D*). A ring of four chromosomes, although theoretically possible, has not yet been met with.

The explanation of the facts appears to be as follows: in the animals doubly heterozygous for the translocation chromosomes III and IV have interchanged their distal ends. In the vast majority of the cells two separate bivalents are formed, each of which possesses a single chiasma in the proximal region (White[6]). In in few cells pairing has

taken place as in Figure 10*A*. This has permittd the formation of a chiasma in the translocated region. Theoretically one might expect that a second "translocation chiasma" might sometimes occur in the position marked by the *Ch. n* in Figure 10*A*; thus leading to the production of a ring of four chromosomes as in the Trimerotropis case. The low frequency of occurrence of chiasmata in the translocated regions is probably in part due to the fact that the chiasmata in Metrioptera are localized.

The investigation of this translocation is admittedly incomplete at the present time, but the facts seem worth recording as they stand. Apparently the regions interchanged are approximately the same length. This is very unfortunate, since it is consequently not possible to distinguish animals homozygous for the translocation from those lacking it altogether. It is hoped that future work will help to answer the question: what proportion of the population are heterozygotes? Another question which is of considerable interest is the determination of the geographical distribution of this chromosomal aberration — will it be encountered elsewhere in the New Forest, or in other localities in Southern England: The fact that *Metrioptera brachyptera* is confined to boggy places on uncultivated land (i.e. that it has a highly discontinuous distribution) renders this question of particular interest.

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✦

Morbid Inheritance In An Isolated Rural Community

TO those who feel that heredity plays but a little rôle in medical practice this article should be an eye-opener. McFeeter* notes a family in which 21 persons had cerebellar ataxia through four generations. One mother who died too young to show the ataxy nevertheless transmitted it to two of her children. Five daughters of an ataxic mother had fits in infancy and strabismus. They are too young to show the ataxy as yet, but the question is raised as to whether the fits and the strabismus may in these cases be the forerunners of the ataxy.

Another family had seven cases of progressive muscular dystrophy of the pseudo-hypertrophic type. As is usually the case, the disease attacked the males and the defect was transmitted by the unaffected mothers. Despite the apparent "familial" condition, six of the seven cases had been previously diagnosed as tuberculosis spine. The family history should have suggested a sex-linked recessive condition of some type to the practitioner.

Tonsilitis might be regarded as due to environmental rather than hereditary causes, but McFeeters finds that this condition runs through four generations in one of his families and, contrary to what would be the case if it were dependent upon environment, affects on the average only half the children of an affected parent, although all children share the same surroundings.

Otitis media is inherited as a dominant also in some of his families, and is not related to tonsilitis, the two conditions running separately.

Inguinal hernia was relatively infrequent in the community in view of the heavy farm labour carried on by the men and most of the women. Fifteen of the twenty cases in the community were limited to two families. In two instances affected persons married. One such mating produced six children of whom five had hernia (two being bilateral). The second such mating produced one living child with double inguinal hernia.

Cancer of the colon was limited to four families in the community, with one exception. In these four families the condition was common.

Rodent ulcer occurred in high proportion in the village, and again its incidence was rather sharply marked in some families.

Hydramnios is a frequent accompaniment of malformation, and it may have been the malformations which were the hereditary factor here. The woman had an anencephalic fetus and then two normal sons. One of these had a child with patent ductus arteriosus; the other an anencephalic male child and a normal daughter.—Madge Thurlow Macklin in *J. Can. Med. Ass'n.* August, 1939.

*McFeeters, J. W.: *The Lancet*, 1:218. 1939.

BRACHYDACTYLY IN THE FOWL*

Is it Linked to Leg Feathering?

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BRACHYDACTYLY is a skeletal variation in the bird which has received little attention. Danforth[1] made some study of the developmental and genetic relations of brachydactyly in the fowl and Schmalhausen[2] proposed that the growth of feathers on the tarsus interferes with normal development of the fourth toe, thus accounting for the association between tarsal feathering and brachydactyly. In the fowl brachydactyly is expressed in varying degrees of shortening of the fourth (outside) toe. In the normal foot this toe is about ten per cent longer than the second (inside) toe, according to Danforth. The toes of 22 Barred Plymouth Rocks, 50 S. C. White Leghorns, and 25 S. C. Rhode Island Reds were measured by the writer and the outside toe exceeded the second toe by 13, 11 and 12 per cent, respectively. These breeds are free from brachydactyly. The expression of brachydactylism is quite variable ranging from instances where the fourth toe is equal to the second toe to a much shortened fourth toe having only one or two phalanges. The two feet were usually in close agreement but on a few birds the grade of brachydactyly differed on the right and left side.

Danforth found brachydactyly expressed in several breeds having leg feathering. The present study deals with the character as found in various crossbreeds used in genetic studies at the Kansas Agricultural Experiment Station. The original brachydactyls were derived from some Light Brahma stock and Silkie Bantams.

In this study the condition of the foot was recorded in four major classifications—the normal where the fourth toe was the longer; those having the fourth toe equal to the second (grade 1); those with fourth toe slightly shorter than the second (grade 2); and those with conspicuously shortened fourth toes having at least the nail missing (grade 3). The last three groupings were all considered types of brachydactyly.

The classification was made by matching the fourth and second toes of a foot and not by actual measurements. This was accomplished by inspecting the foot dorsally with the two outside toes held together. Classification was made on day-old chicks and checked again at eight weeks.

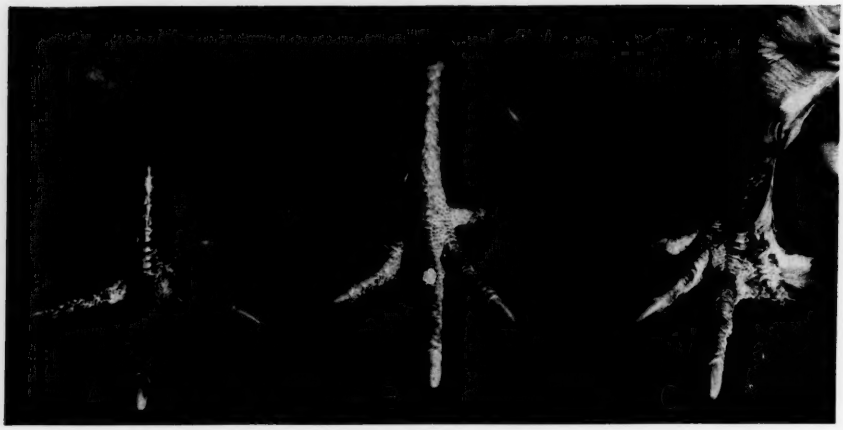
Influence of Sex

Of the 1,383 brachydactylous females examined (Table I) 55 per cent were in the class having the fourth toe equal to the second, 37 per cent had a slightly shorter fourth toe and eight per cent had a conspicuously shortened fourth toe. In the 1,367 males, the percentages in the same groupings were 41, 48 and 11 per cent respectively, indicating that the males are inclined to show brachydactyly to a slightly greater degree than the females. This fact is also brought out by the data presented in Table II.

Association with Tarsal Feathering

The association between tarsal feathering and the brachydactylous condition of the fourth toe is clearly shown in Table I. The birds with normal toes seldom showed any degree of tarsal feathering, 95 per cent of the females and 99 per cent of the males having unfeathered legs.

*Contribution No. 114, Department of Poultry Husbandry.



COMPARISON OF NORMAL AND BRACHYDACTYLOUS FEET

Figure 11

The normal foot (*A*) shows no leg feathering and the fourth toe (to the left) is longer than the second one. The extreme brachydactyl (*C*) shows the fourth toe with the three distal joints missing and the leg well feathered. The middle foot (*B*) probably belongs to a heterozygote, having slight leg feathering and the fourth toe (left) shorter than the second toe.

Only a small percentage of the birds showing any degree of brachydactyly were free of both tarsal and foot feathering. Some of the exceptional individuals in both this and the group of normal-toed birds may be errors in description. Since all degrees of relative length of the outside toe were found, an occasional equal toed bird might be classed as normal and very slight leg feathering in a brachydactylous bird might be overlooked. At the time of classification chicks were identified by wing band only, without knowledge of the type of mating from which they came. Danforth noted that in some brachydactylous birds showing little or no tarsal feathering, the scales on the side of the leg or foot were irregular. The classification—slight tarsal feathering—usually included birds for which the feathering was light and largely restricted to the foot. The well-feathered tarsus ranged from one similar to that of a Langshan to degrees approaching but seldom equalling that of the standard Brahma.

From Table I it will be seen that there is a tendency for the degree of tarsal feathering to vary inversely with the relative length of the fourth toe. The percentage of non-feathered legs decreases as the fourth toe becomes shorter. The percentage of birds with well-feathered legs also increases from grade 1 to grade 3 of brachydactyly, the values being 51, 68 and 84 per cent, respectively.

Breeding Results

The breeding results in Table II indicate that brachydactyly behaves as an incomplete dominant where most of the heterozygotes are distinguishable from the homozygous dominants. In classifying the offspring all birds showing any degree of shortening of the outside toe were listed as brachydactyls. The data agree closely with the theory that birds with the nail or phalanges missing from the fourth toe (grade 3) are homozygous dominants (*BrBr*). The normal toed birds with the fourth toe exceeding the length of the second toe are the recessives (*brbr*). Those with the fourth toe equal to or less than the length of the second toe but with the nail present are mostly heterozygotes (*Brbr*). The number of birds tested was not large enough to indicate accurately the proportion of heterozygotes and homozygotes in this group.

The mating of three *brbr* (normal) males to normal females produced only normal offspring as would be expected. Three of the five normal males used were also mated to grade 1 and grade 2 females. If such females were *Brbr* then the offspring should be half brachydactyls and half normals. In these mat-

ings the 1:1 ratio was sufficiently closely realized. This is not true, however, in two out of three of the matings of normal males to grade 2 females. There is an excess of brachydactyls suggesting that some of the several dams in each mating may have been homozygous for brachydactyly.

The matings of the two grade 1 males to normal females further indicated that these males were of the constitution *Brbr*. The additional mating of one of these males (1484M) to grade 1 females should theoretically produce three brachydactyls to one normal but it is seen that the actual ratio is 29 to 45. There is here a considerable shortage of brachydactyls.

Seven grade 2 males were included in the matings and four of them (1476M, 1490M, 1522M and 1536M) appeared to be homozygous for brachydactyly, while the other three gave ratios indicating that they were heterozygous. Most of the *BrBr* grade 2 males, as was true of the grade 3 *BrBr* ones, threw a few individuals classed as normals when mated to normal females. This must mean that a few heterozygotes vary to the degree that they are difficult to distinguish from normals.

More critical evidence regarding the mode of inheritance of brachydactyly is presented in Table III. Here the same group of rather extreme brachydactylous females were mated successively to three types of males in the same season. One male was normal *brbr*, another was grade 1 *Brbr* and the third was a grade 3 *BrBr*. The females were assumed to be all of the constitution *BrBr*. As was to be expected no normal offspring appeared. The resulting progeny were classified as to the grade of brachydactyly.

When the *brbr* normal male was mated to the *BrBr* females the offspring were all of grades 1 and 2. This was in agreement with the earlier evidence that these two grades are largely heterozygotes. In the females grade 1 predominates while in the males the reverse is true supporting the earlier evidence that males show brachydactylism to a greater degree than females.

The mating of the grade 1 *Brbr* male to these females should produce one-half *Brbr* and one-half *BrBr* offspring. The data in Table II indicate that all grade 3 birds are of the constitution *BrBr* but that not all *BrBr* birds are extreme enough to fall in grade 3. Thus it would be expected that the total of grades 1 and 2 offspring would exceed the grade 3 when a *Brbr* male is mated to *BrBr* females, since some of the homozygous brachydactyls will be classed as grade 2. In Table III the grade 3 offspring total 21 while the grades 1 and 2 total 40.

The last section of Table III supplies further support for the view that some homozygous brachydactyls are classified as grade 2 instead of grade 3. Here a grade 3 male was mated to grade 3 females and the resulting offspring totaled 148 grade 3 and nine grade 2 brachy-

Table I. The relationship of tarsal feathering to the brachydactylous condition of the fourth toe.

	Well-feathered tarsus	Slightly feathered tarsus	No tarsal feathering	Total	Percent non-feathered
Females					
Normal toes	5	11	329	345	95
Grade 1 - 4th and 2nd toe of equal length	358	310	92	760	12
Grade 2 - 4th toe slightly shorter than 2nd	334	157	26	517	5
Grade 3 - 4th toe conspicuously shortened - nail or phalanges missing	100	6	0	106	0
Males					
Normal toes	4	2	422	428	99
Grade 1 - 4th and 2nd toe of equal length	254	240	60	554	11
Grade 2 - 4th toe slightly shorter than 2nd	428	197	37	662	6
Grade 3 - 4th toe conspicuously shortened - nail or phalanges missing	123	23	5	151	3

Table II. Genetic results of matings between birds of various grades of brachydactyly and between brachydactyls and normals.

Male Number	Male's Grade	Grade of Brachydactyly shown by his mates*					
		Normal		Grade 1		Grade 2	
		Offspring		Offspring		Offspring	
		Nor.	Bra'y.	Nor.	Bra'y.	Nor.	Bra'y
1477M	Normal	125					
1492M	Normal	184					
1520M	Normal			18	21	5	22
1527M	Normal	41		69	84	27	31
1537M	Normal			48	55	11	33
1484M	1	88	87	29	45	6	32
1539M	1	54	58				
1475M	2	87	85			7	13
1476M	2	6	192		11		59
1479M	2			55	114	13	34
1485M	2	38	27				
1490M	2		158		42		14
1522M	2	3	367				
1536M	2	7	321		50		
1494M	3	3	74	1	41	1	53
1505M	3		34		13		43
1563M	3	2	86				

*Normal = non-brachydactylous, fourth toe longer than the second; grade 1, fourth and second toes of equal length; grade 2, fourth toe slightly shorter than the second; grade 3, nail and phalanges missing.

Table III. Grades of brachydactyly in offspring when grade 3 brachydactylous females are mated successively to normal, grade 1 and grade 3 males.

Dam No.	Sire 1557M-Normal			Sire 1526M-Grade 1			Sire 1535M-Grade 3		
	Grade of brachydactyly in female offspring			Grade of brachydactyly in female offspring			Grade of brachydactyly in female offspring		
	1	2	3	1	2	3	1	2	3
5518	3				1				1
5519				7	2	3			8
5520	12	2						2	3
5521	3	6				1		1	14
5522	11	10		2	3	3			10
5523	3					1			12
5524	2	2		3	5	1		1	16
Total	34	20		12	11	9		4	66
	Grade of brachydactyly in male offspring			Grade of brachydactyly in male offspring			Grade of brachydactyly in male offspring		
5518	4	7							7
5519		1		3	3	6		1	9
5520	7	10						1	3
5521		8				2		1	9
5522	9	14		3	5	3			22
5523	2	8		2				2	11
5524	5	2		1		1			21
Total	27	50		9	8	12		5	82
Grand Total	61	70		21	19	21		9	148

dactyls. Theoretically these should all have been of the constitution *BrBr*.

Discussion

From the data here presented it would appear that brachydactylism is due to an incompletely dominant autosomal gene. The homozygous dominants always have the nail and some of the phalanges missing from the fourth toe. The heterozygotes and some of the homozygotes show the character in an apparently complete but shortened fourth toe.

The close relationship between brachydactyly and leg feathering suggests two possible explanations: they may be independent factors but with their genes closely associated on the same chromosome, or they may be different expressions of the same gene. The available evidence does not conclusively support either alternative. In the stock used in this study leg feathering had been carried in a heterozygous state for several generations without any consideration being given to brachydactyly. In most instances both leg feathering and brachydactyly were incidental to other characters being investigated. It would seem that unless the two factors were very closely associated on the chromosome more instances of their independent expression would have been encountered since they had been carried in a manner favoring their dissociation.

The fact that the degree of leg feathering varied inversely with the shortening of the fourth toe appears to support the view that these two characters are merely the expression of the action of one and the same gene. It is true, however, that this interpretation cannot be applied to leg feathering as it is found in the breeds exhibiting it. If one gene determined the expression of both, a large percentage of the birds in such breeds as Cochins and Brahmas would show the extreme degree of brachydactyly. The writer believes that the possession of outside toes with some joints missing is common in these breeds but the incidence is not high enough to permit a monofactorial interpretation. It has been suggested that heavy leg feathering is due to two dominant factors. It is possible that one of the two factors might produce brachydactyly along with leg feathering. If that were true, breeders would probably select against homozygosity for this factor in order to reduce the incidence of extreme brachydactyly in the breed. Other more critical studies are in progress to determine the relationship of brachydactyly and leg feathering.

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Jobs for Animal Geneticists

The United States Civil Service Commission announces open competitive examinations for Senior Animal Geneticist, $4,600 a year; Animal Geneticist, $3,800 a year; Associate Animal Geneticist, $3,200 a year; Assistant Animal Geneticist, $2,600 a year. Existing vacancies in these positions in the field will be filled from these examinations, unless it is found in the interest of the service to fill any vacancy by reinstatement, transfer or promotion. The salaries named above are subject to a deduction of 3½ per cent toward a retirement annuity.

Applicants must possess the following qualifications: 1. They must be citizens of the United States on the closing date specified; 2. For positions in the opportioned service at Washington, D. C., they must show legal or voting residence in the State or Territory claimed for at least 1 year next preceding the closing date for receipt of applications; 3. *Education*—They must have successfully completed a full four-year course leading to a bachelor's degree in a college or university of recognized standing; 4. *Experience*—Except for the substitution of additional education for experience, provided for below, they must have had, subsequent to the completion of the required college course, the following experience: Senior Animal Geneticist—At least six years of responsible and successful research experience in some phase of animal genetics. The other grades must have had at least 5, 3 and 2 years experience, respectively. Applications must be on file with the United States Civil Service Commission at Washington, D. C., not later than April 22, 1940, except for residents of certain western states, who may file by April 25, 1940. For full details write U. S. Civil Service Commission for announcement regarding these examinations.—"No. 24 Unassembled," for above date.

FIVE LITTLE DIONNES AND HOW THEY GREW

Some Notes and Reflections on the Effects of Two Million Visitors and Three Quarters of a Million Dollars on the Problem of Evaluating Heredity and Environment

GLADYS C. SCHWESINGER

ON MAY 28th last, the quintuplets celebrated their fifth birthday. A week before, they had been presented to Their Majesties, King George and Queen Elizabeth of Britain, while a waiting world held its breath to see how the little girls would conduct themselves on so august an occasion. One cannot be too sure of five-year-old decorum at any time; still less can one count on five five-year-olds all observing the amenities of a court presentation at any one time! Yet these little girls, who had never before left the enclosure of the Dafoe nursery, behaved with the composure and self-control of maturity, while sacrificing none of their childish charm. If there is truth in the old Jesuit adage—since revived by the Watsonian behaviorists—that as a child's character is shaped in the first five years, so will it be inclined for life, then we can anticipate that the Dionne quintuplets will carry with dignity and sweetness the burden of publicity and continued adulation which is to be their lot.

Already they have been observed from behind a one-way screen by nearly two million visitors who have journeyed two hundred miles north of Toronto, just for the purpose of looking at them from a discreet distance. But to date, the quintuplets are scarcely aware of their own importance, even though they are wards of the crown; even though they have already amassed an estate worth three-quarters of a million dollars, and one which is steadily growing, depressions and recessions notwithstanding; even though they are the center of heated controversy between exponents of French-Canadian nationalism, progressive education and formalism, religious denominationalism, commercial interests, and personal animosities and bickerings. Various factions are agitating for supervision of their rearing, for determination of their lingualism, and doubtless also for control over their property.

Born in poverty, prematurely, without benefit of prenatal medical attention, in the remote Canadian hinterland, in an area which is served by one doctor per 400 square miles, they have become, even in pre-school years, the glamour girls of the world. Their income has reduced the poverty of their parents to a state approaching affluence in their own community; has sent their older brothers and sisters away to boarding-school, and has enabled their parents to own an expensive automobile. Their coming has given employment to a large staff of attendants at the hospital: nurses, police guards, teachers, servants; and has brought business to their neighbors in the way of fulsome service at filling-stations, over-night camps, concessions for selling souvenirs, and the construction by the Government of a fine three-lane highway to their door.

Why all this acclaim over five little girls? Is it possible that in this concentration of public attention, there may be a sort of sublimated expression of a culturally suppressed urge to fertility? An urge which once received overt religious sanctions and support, but which today is set aside in the interests of other demands of the social organism? Does this public enthusiasm represent only intense appreciation of medical skill, or may it not be something in the way of a vicarious wish-fulfillment of the Dionne biological achievement? Why, for instance, do so many visitors pick up and carry away those small stones which nature has sprinkled so generously on the

not overly fertile Dionne acres?

Not a little of the public interest, however, must be credited to what we shall call educational curiosity. The material changes in the Callander landscape, brought about by the business expansion, are insignificant as compared with the stimulation which the Callander experiment is giving to a constantly watching world in bringing home a realization that continuous right care and supervision should not only develop bigger and better babies, but should also mould more wholesome and charming personalities.

At first, the big question which everyone asked regarding the quintuplets was: Will they live? The services of a devoted country doctor, a ready-to-act Red Cross unit, a generous humanitarian medical and lay world, and the best of nursing care, insured that the highly unpromising bits of humanity—described by an observer as initially looking like "skinned squirrels"—did live and thrive. Science proved once more that, given the means, it could be done! Of the forty-five quintuplet births on record, only one group ever lived as an entity for as long as one hour. In another group, only one of the five lived for as long as fifty days. The little Dionnes spring from a virile rugged Norman-French stock, settled for 300 years in Canada. Among the factors determining their survival, their sturdy physical heredity was no minor contribution. Neither this nor their germ-free, pure-air northern environment should be overlooked.

The story of the physical growth of the quintuplets is an open book. Every ounce of weight gained had its news value. But what of their psychological development? Reassured of their chances for life, many people began asking the next big question: Will the quintuplets be normal human beings? How will the rearing which they are getting—itself, a new kind of institutionalism—affect their personalities? May not their early ill health, their prematurity, their multiple-birth origin, have retarding effects on their mental development?

These are interesting questions, and the askers had to wait for the answers, and are still waiting for some of them. Even those individuals who went in person and viewed the babies through the large one-way screen, placed around their play arena, could not be sure. The toddlers, at the early age of two and three years, played actively and happily enough—but they did not talk! This language retardation looked a bit discouraging. What did the child psychologists, those specialists in matters of intelligence development, have to say about the mental development of the quintuplets?

The Dionne Day

Fortunately for the record, when the babies were ten months old, Dr. W. E. Blatz, professor of child psychology at the University of Toronto, and Director of St. George's School for Child Study, was called in to observe the mental and emotional growth of the quintuplets, and to supervise their pre-school education. Experienced in nursery-school methods, Dr. Blatz and his staff emphasized regularity of regimen for the babies. The daily schedules were not allowed to vary except as they were made to adapt to the growing needs of the children. Prayers, bath, orange juice, dressing, breakfast, nursery play, outdoor play, lunch, nap, more outdoor play, recreation, bath, informal visit with members of their family, supper, prayers, bed, constitute the high spots of their schedule, with plenty of opportunity for freedom of expression in play and social activity in their little world.

Dr. Blatz kept records carefully from the time he entered the nursery, when the children were ten months old, up to the time he withdrew, when they were three years of age. During this interval, the children were tested and measured and rated and observed through the medium of various techniques in the hands of trained psychologists and nursery-school experts. The scores and ratings of the quintuplets are available to the initiated. The question once asked: Will they be normal? now gives way to the question: How do the quintuplets compare with ordinary children in their development?

At three years of age, it could be seen

that in some respects they were still retarded; in others, advanced. Retardation was most noticeable in language; and even at five years, it still is. This is not surprising. It has been found that twins, who have each other to converse with, do not approximate single children in language acquisition, being retarded by from four to eight months. The quintuplets are as much behind twins, as the twins are behind singlets. A child's need to use words lessens with the multiplicity of companions of his own age, who talk his own baby language. Gesture and baby language served the quintuplets adequately for most of their social communication at a time when ordinary children are already articulate in speech. Even the adults around them understood their well-developed gesture talk. In a medium in which needs are being constantly anticipated, as they are for the quintuplets, there is little stimulation for the production of words for stating those needs. The bilingualism at the Dafoe Hospital may also have served somewhat as a deterrent to verbal growth. Even today, at five years, the quintuplets do not know as many words as do ordinary children.

Another skill, which was relatively late in appearing, was the first walking steps—which were taken at the age of 15 months, as compared with the average of 12 months for babies at large. But this early retardation in a motor skill appears to have been overtaken by now. Today, the little girls are well developed physically, being 30 per cent heavier than the average child of their age. The quintuplets, at five years, weigh from 50 to 54 pounds as compared with an average of 37½ pounds, recorded for children in the U. S. Children's Bureau data. The quintuplets are larger than their next older sister. They get 13½ hours more sleep per week than does the national sampling of American children! They are skillful with the paint brush and carpenter tools, as with tricycles and jungle gym and other play equipment, not excluding skis!

Mind and Feeling

Mentally, when tested at the age of three years, the children fell a little in arrears of the test norms in "adaptive behavior" items, except when their own environment provided experience in the item of behavior called for, such, for instance, as opening doors, in which performance they excelled. In personal and social behavior they were well along, even at three years, presaging their successful later debut at Court! All five were about equally developed socially, meeting people with ease, exercising becoming self-restraint in social situations, and behaving harmoniously among themselves. Today, they have a warmth and a charm which never fails to endear.

They do not become "emotional" when thwarted by toys which stick or wheels which jam; they are not given to nagging or quarrelling or whining, although, at times, they tease and jostle one another, just as all children do who play together. They have never frightened one another. Indeed, they have experienced relatively little fear in their lives and face newcomers with a confident trust which is part of their charm. Needless to say, fear has never been used as a means of discipline, and they have had ample opportunity to adjust to strangers, doctors, dentists, new teachers, august visitors, police guards, and so on. This is all good foundation for stabilizing a self-confident personality adjustment to the eager public whom the quintuplets will be called upon to face as adults.

It will be interesting indeed, some fifteen or twenty years hence, to look for correlations between their poised behavior today and the degree and kind of social adaptability which they will display then. The Jesuit adage can be put to test in the laboratory of their lives. Will they then be similarly self-sufficient socially? Will they be able to respond with equanimity to the demands to be made on them as adults? The relation between childhood traits and later adult adjustment (especially when environment has been subjected to continuous observation and check) is only one of many questions of particular interest to the psychology of personality.

Limits of Development

Of what other special interest are the quintuplets to science? This questoin readily breaks up into two categories:

(1) What are the possibilities and limits of mental and physical development through maximal opportunity and the best possible kind of rearing, at least insofar as we can determine what these most favorable influences are? The answer to this question becomes the concern of parents, educators, eugenists, psychologists, and child specialists everywhere.

Although well supplied with culturally favorable items, we do not hold that the environment in the Dafoe nursery is necessarily the "best possible" psychological world in which to rear children. Indeed from the psychological point of view it leaves too many things to be desired. There is, for instance, the lack of personal contact with the outside world. The quintuplets do not leave home from time to time, as do ordinary children, to enrich their experience by going to places and seeing things previously unseen. They lack the opportunity for that kind of social adaptation which comes rather early to most children, from rubbing shoulders with "unselected" individuals. The child who leaves the shelter of his own backyard and plays in the neighbor's must learn to stand on his own feet, without benefit of maternal favor or special family privilege. He must learn early to "take" what circumstance, relatively unguided, provides for him, even when that circumstance differs considerably from the more sheltered precincts of home.

Children grow as they intermingle with other children whose experiences have been different from their own. Not so with the quintuplets. Although well supplied with companions of her own age, each child in the quintet has no unique experience to offer to the others. For play suggestions, each quint-member is compelled to draw from a reservoir of episodes and ideas which is constant and common to them all. Variations in behavior may happen by chance, and these may be capitalized upon when they occur. But the outcropping of original ideas by this means can be expected to take place slowly. Originality is an outgrowth from a base of accumulated data, facts, ideas, experiences or working patterns which others also have contributed to one's stock in trade. Through social interaction vicarious experience is built up; through mingling with individuals of different background from one's own, horizons are extended; new points of departure are provided. The Dionnes, so far, have lacked these short-cuts to self-initiated living and learning.

When will the quintuplets be allowed the opportunity to find their places in relation to other children whose life patterns will be different from theirs,—whose play demands and adaptations will be of another sort? When can they experiment in a necessary life function, that of exercising initiative in an environment not tailor-made to their order? When are they to experience that hardship, that frustration, that casual circumstantial discipline by which character is built? When are they to encounter the hurdles of life by which weakness becomes surmounted through continuous practice in meeting obstacles? The discipline of having to adapt to human relationships, not ideally chosen, comes early to most of us. Certainly, for most of the children on this continent, the need to compromise with people and conditions as they are, outside of the home, comes by the sixth year. By the time the average child enters school, he has already taken his first steps in establishing this compromise.

Not that the little Dionnes can be called socially immature. Far from it. By the time they were three years old, under the guidance of the trained nursery staff, the little girls obtained a level of development in social attitude and physical skills, much in advance of what they would have attained, had they, like Topsy, "just grown." One has only to contrast their open friendliness with that of their retiring brothers and sisters and so many rural children, to realize to what an extent an extroverted personality can be encouraged to emerge. But except

for the single exception of their trip to see the King and Queen, these adaptations have all been learned within the constant environment and always within close range of supervision.

(2) The second question of major interest to science is: How much variation in traits will appear when five identically similar organisms are reared in the one highly controlled environment? The answer to this question will throw its quota of light on the much thrashed at heredity-environment problem.

The identicalness of heredity for the Five has been definitely established on the basis of evidence obtained by the obstetrician, who assured his medical colleagues that "It was definitely a single placenta." (4, p. 675.) The physician's observation was supported by the two attending midwives. Later the data obtained by Dr.'s Ford and MacArthur of the University of Toronto, on finger, palm, toe, and sole prints were sufficient to determine uni-ovularity. Observations as to hair whorls, free lobing of the ears, facial resemblance, all added their weight of support to an assured diagnosis of uni-ovularity. The left hands of Marie and Cecile are more alike than either left hand is like its own right. Yvonne and Annette's hands are also similar, with Emilie bound in similarity to the others, the whole set of five being linked. It probably was a case of the original zygote doubling, each new one redoubling, with one of the four again doubling, until five were under way. Or it may be that there was a sloughed-off sixth embryo, but this speculation is not at all necessary to establish original uni-ovularity.

In a recent paper in this JOURNAL[5] MacArthur and Dafoe established the fact that the quints, in spite of differences in birth weights, were growing more and more alike in both weight and stature. The inequalities of the prenatal environment restricted the fullest expression of their hereditary growth factors; the subsequent equality of the postnatal environment considerably aided the originally handicapped members to catch up on the more favored members of the quintet, but not entirely to overtake them. Marie, for instance, is still the smallest and weakest, the "baby" of the group. Yvonne is considered the most mature in her bearing, playing the role of "older sister" to the others.

This suggests that the original physical differences at birth served to point out directions for the personality to take in the case of each member of the group. Dr. Blatz tells us that Annette is socially aggressive, as compared with the others; that Cecile is socially interesting to her mates; that Emilie is independent and self-assured, even magnanimous. These statements were made when the girls were under four years of age. Differences in trend are not crystallized so early into personality clusters that will necessarily remain fixed. But they are indicative of the fact that the influences which make for personality variations are not the static but the dynamic aspects of one's social milieu, the subtle interplay of person on person, with a residue of change which each social encounter leaves on the personality as a further foundation for the next inrush of experience. When the range of social contacts or field of activity is curtailed, then the impact with each person or each experience within the more limited range is that much more significant and determining—as for example what hospitalized patient has not observed, especially if he is confined to his bed: The smaller the range of social encounters, the greater the indentations of each person's behavior upon the others. It is not surprising — as some would think — that each quint has developed a fairly well-marked reaction-pattern to the others. Nor should it occasion further surprise if these habits of the one to the other become more firmly fixed with time and innumerable repetitions of the behavior. The very expectation that this will be so is one big argument for bringing other children into their lives, not only to give the quints the right conceded to every child within limits to choose his own friends, but to supply them with a variety of child-personalities to whom they can learn to adjust, and from whom they can derive satisfactions, modifications, and compensations by setting up rela-

tionships, each according to her particular need.

A socially aggressive child, for instance, can in time become bumptious if he does not encounter others with as much or even more of this trait; a "baby" will remain a baby if always cast in that role, and not given the opportunity to be superior to someone else; and so on for each trait or relationship which may become developed within a closed group. If the door of the Dafoe nursery is not soon open to a wider opportunity for the Dionne quints to find their roles in respect to other children of their own age or closely related ages, then we can expect the "personality differences" which have already been noted to become deeper, more decisive, and probably permanent.

The "personality differences" which Dr. Blatz has recorded are concerned chiefly with the reactions of the quints to one another. They are probably true manifestations of the self which is emerging within each little person. They may not be indications of how each child will react to other personalities. My guess is that in this situation their behavior, outwardly at least, will reveal a surprising degree of similarity. Already we know that to adults, who are kind and friendly, the girls comport themselves with charm and poise. Already we know that they are highly similar in physical and mental abilities. In adducing evidence to show that each girl has a personality of her own, the observers have probably magnified the differences, and minimized the similarities. I suspect that the latter are preponderantly greater.

It is comforting to know that each little girl is accorded the right and the encouragement to be herself, and not just one-fifth of a set. It will be within these separate selves that developmental variations will be reflected, much more than in any outword performance of conduct or behavior or skill. Science will want more data as these children continue to grow into their adult selves, difficult as it will be to secure these data. (Unfortunately, at this point, we regret to report that the relatively liberalized educational regimen of the nursery has been supplanted by a more highly formalized one, which on the face of it would evoke more and more conformity to a common behavior pattern.)

The difficulties confronting the guardians of this precious human trust are vast and perplexing. We are not assuming the right or the privilege of criticising their procedures or plans for rearing the children. But we are interested in the implications of whatever plans they do select. We are concerned that the unique data, inherent in the world's only quintuplets, should not be lost to science, especially when their accumulation works no hardship on the subjects. We hope that trained psychological observers will have further opportunity for recording details about the processes of development in physical, mental, and personality areas of the quints.

It may be that the "mental reservations," with which people in the public eye have to surround themselves, will mean that only through close and confident cooperation with sympathetic scientists will the real selves of the Dionne quintuplets ever be revealed. That remains for the future. What we must be content with today is satisfaction in the thought that close adherence to the principles of child-rearing, as enunciated by experts in the field of medicine, results in the production of healthy, happy, and physically well-developed children. That adaptation to the principles of child psychology and preventive mental hygiene —even in a necessarily restricted social environment—produces stability of personality and the capacity for inter-personal harmony, qualities the world so sadly needs.

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EUGENIC EDUCATION IN COLLEGES

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THE program adopted by The American Eugenics Society should be pushed to the attention of young people who are in the later years of youth or in early adulthood. Thousands of these young people are in colleges after graduation from high schools, but the majority are not in colleges and some of them can be reached through Adult Education as it is now developing in many parts of America. This paper is limited to eugenic education in colleges, but the essentials of the program which I am going to propose for college students may be adapted to individual or group study by serious young people who do not go to college.

After many years of discussion, this Society has now emerged with a definite and conservative program for American eugenics. The time has come for pushing that program in American education, and the greatest opportunity is in institutions of college rank in the United States, of which there are about 1,700. Many of these colleges have curricula which provide for liberal or cultural studies with which eugenic education may be logically associated.

A very small percentage of American college graduates of recent years have acquired during their four years in college any clear conception of the meaning, the general principles, and the importance of eugenics as stated in the program of this Society. My informal testing of many hundreds of graduate students and of college-educated adults indicates that the great majority think that eugenics means sterilization of mental defectives and little or nothing more. On the other hand, a very respectable minority thinks that eugenics means only genetics applied as recommended in agricultural books on animal breeding.

For this unsatisfactory situation we must place a large part of the blame on college professors of biology and genetics, who have claimed a monopoly of eugenics as an advanced application of the science of genetics. From this it has followed that few college graduates have studied eugenics, as the great majority of college students do not take advanced work in biology which would include special courses in genetics. This leads me to offer for discussion four propositions:

(1) Eugenics in colleges should be presented as a unit of study from the viewpoint of social biology (biology and society), and preferably by a teacher who is primarily interested in human affairs either as a social biologist or a biologic sociologist. Instruction concerning eugenics should be popularized in content and should be open, *without departmental prerequisites,* to all juniors and seniors and to certain mature sophomores who receive special permission. In short, eugenics (like personal hygiene) should be presented as a unit of instruction planned for the overwhelming majority of students, and not "just another course" which adds to the glory and enrollment of any particular department.

At this point there are objections from many professors of biology who seem to have a limited understanding of the up-to-date eugenic program and who are victims of the departmental jealousy which is the curse of most American colleges. From these biologists we hear that "eugenics can not be taught without a prerequisite course in genetics," and that "a first course in genetics must be based on at least ten to fifteen credits of college biology."

Such objections are based on a profound misunderstanding of what the average intelligent citizen wants to know about eugenics. I have followed this latter question for over twenty-five years in colleges and with adult groups outside, and my answer is that the biological foundations and the elements of heredity which most interest the average citizen who has had little science study can be made quite clear on the basis of less than a dozen hours of lantern-illustrated lectures planned for those who have had little or no instruction in formal biology.

(2) Eugenics should be placed in the college curriculum so as to reach the maximum number of maturing students, especially in the last two years. I no longer believe it wise to offer a popular short course in eugenics under that name. If time is limited and eugenics cannot be presented as suggested in the next paragraph, I would compromise on a short course without prerequisites and entitled "Human Heredity, Environment, and Eugenics."

(3) Although I am a biologist, I have come to believe that the best setting for eugenics in any undergraduate college is in connection with general courses on the family, or in social biology presented as an orientation course, and not in connection with any of the regular courses in biology. More than 300 four-year colleges now have courses on the family, and there is a tendency towards an interdepartmental or cooperative course, with participation of professors of biology, health, sociology, psychology and home economics. In such a setting a unit on eugenics has its natural place as an integral part of study of the family as the unit of our society. I would name such a unit "Eugenics and Family Betterment." Moreover, eugenics thus presented will reach many more students than would select an advanced course under the department of biology.

In addition to a unit of instruction on eugenics as recommended above, there are many other courses where at least reference to or general approval of eugenics is logical. Recent surveys indicate that various courses in sociology, psychology, home economics, ethics and hygiene contribute much concerning the family as the natural center of all normal relations of the sexes which is stressed in the best social hygiene education.

(4) Family eugenics and not national eugenics should be the center of interest. Eugenics means more to the average intelligent person if based on the family as the unit of society and the center of individual interest. For teaching purposes, we should abandon the old emphasis on eugenics for improving society or the nation and teach family eugenics. It is not useful to the majority of students to present eugenics as primarily a scheme for national reform of the biological basis of human life. Even many reputable biologists question whether national eugenics is not an impractical dream; but certainly family eugenics is workable as now applied in thousands of family groups. The word "society" means something that is more or less philosophic, unreal, and uninteresting to many intelligent adults who have not studied social sciences intensively, but the word "family" obviously requires no explanation or defence in discussion with any man or woman whose intelligence is above average. These remarks suggest reasons for correlating and integrating the study of eugenics in courses on the family.

Summary

(1) The American Eugenics Society should encourage the wide extension of eugenic education in colleges where most of the leaders of tomorrow are now in preparation.

(2) An adequate biological basis for introductory study of eugenics does not demand prerequisites in regular college courses in biology or in genetics.

(3) The best opportunity for attracting the attention of the largest number of college students is in connection with courses on the family which, in response to a widespread stu-

dent movement, are tending to develop rapidly in many American undergraduate colleges.

(4) The best setting for teaching effectively the chief points in the American Eugenics Program is in popularized social biology or survey courses on the famly combining the facts and points of view of biology, the social sciences, and social hygiene.

(5) Eugenics in colleges should be presented from a social-biological point of view, including the essential foundations of biological knowledge and the sociological elements which are inevitably involved in the full development of the family as the unit of society.

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HUMAN GENETICS AND SOCIOLOGY

THIS textbook* by a Professor of Biology at Stanford University is clear, readable, up-to-date, with exceptions sound in its reasoning, but occasionally marred by sociological naïveté. There can be no doubt that there is great need for such a book. There is no doubt further in my mind, though many sociologists would dissent from this view, that there is a crying need that American sociologists should not continue to neglect biological factors in their social analyses as they have in the past. This book should do a great deal to help the situation, provided the sociologists do not close their minds to it, as many of them will. On the other hand, the treatise would have been more generally acceptable had the author sought a sociological collaborator. The author might not only have been saved certain errors in petty details, but the whole treatise would have been enriched by a broader, more mature and undoubtedly sounder point of view.

The book consists of 26 chapters. The first 10 are devoted to the usual discussion of reproduction, the mechanism of heredity, Mendel's laws, sex and heredity, gene linkage, interaction, variation, etc. Chapters 11 to 21 are concerned with the genetic aspects of physical traits, intelligence, race, population problems, war and migration, mental deficiency, differential fertility, disease, insanity, crime, education and government. The mere listing of these shows what an ambitious discussion this is. Naturally the treatment is thin in spots as a consequence.

A notable feature of the book is the use of several tables and charts from Lorimer and Osborn, *Dynamics of Population* and from Cattell's studies on differential fertility and their social effects in England. Professor Burlingame concludes that the Lorimer and Osborn prediction that present population trends are reducing American intelligence on a rough average of 2-3 I.Q. points per century is too conservative because the Haggerty-Nash sample they use was unrepresentative of the American population. It included no immigrant stocks, almost no Negroes, groups of supposed lower intelligence. Burlingame thinks that the Lorimer-Osborn results are thus spuriously low. However, the reader may be reminded that those possessing I.Q.'s of 140 and up were estimated to be declining 12.7 per cent in one generation; those with I.Q.'s of 130-139 were declining 11.4 per cent in one generation; that those with I.Q.'s of 50-59 were increasing 8.8 per cent, and those with I.Q.'s of 60-69 were increasing 5.8 per cent per generation. The change in the average I.Q. is thus obscured by much more profound and far-reaching changes. Professor Burlingame concludes (p. 302) that "The low class of social inadequates is probably increasing in both the United States and England by 15 to 20 per cent per generation." And that the intelligent classes above 140 I.Q. in both countries appear to be decreasing by 15 to 20 per cent in the city and even more in rural England." Cattell's studies are extremely

*BURLINGAME, L. L. *Heredity and Social Problems.* 369 pp. $3.50. N.Y.: McGraw-Hill, 1940.

valuable, but I am inclined to think they have been accepted here too uncritically.

The fantastic claims of Professor Stoddard (University of Iowa) among others, that the environment of an orphanage could and did reduce the intelligence of bright children to that of the feebleminded is properly scotched. (But Stoddard's name does not appear in the Index.) The sensational conclusions of Stoddard are so out of accord with the results of practically all other investigators that it is hoped he will cease befuddling the American people.

Not infrequently Professor Burlingame gets beyond his depth with rather tragic results. For instance on p. 314 he discusses the problem of medical needs. He contrasts the claims of the National Health Survey (without mentioning its name) that 40,000,000 people have inadequate medical attendance with the absurd A. M. A. allegation that only 40,000 are in need. The former he brands a "political claim" and the latter a "scientific claim" resulting from a "scientific survey." All this is balderdash. It is quite obvious that Professor Burlingame in his discussion of socialized medicine has been unduly and uncritically under the influence of medical propaganda. He seems to think that the medical profession alone and not the profession with the help of the consumer of medical services should determine the type of plan finally adopted in this country. This is only one instance of sociological naïveté which crops out in a score of places in this book. On the whole, the author recognizes the need for some new plan, yet he presents a novel argument against the democratic expansion of medical services, namely, that it would increase the stupid in the population by about 10 per cent. Even if that point is granted for the sake of argument, it does not prove, as perhaps Professor Burlingame would admit, that some intelligent plan of medical service for all is not needed.

Occasionally silly questions are raised and answered in all seriousness. Is this necesary in a textbook. I cite but one example: Are [anti-scientific medical] cults of this sort [chiropractic and Christion Science] due to genetic deficiencies or to an unsuitable environment?" (p. 312). Such questions are seriously answered throughout the book. The book is replete with many doubtful statements in which difficult and complex matters are too briefly treated.

Burlingame's attitude toward the Negro and immigrant is not particularly enlightened sociologically. For example, he discusses the following solutions to the Negro problem: "(1) biological isolation, (2) hybridization, (3) reduction in net fertility substantially below White rate, (4) emigration" (p. 248). One might have supposed that it would have occurred to a biologist that we might try decent treatment and then attempt to live side by side with the Negro. The same limitation of viewpoint pervades the discussion of other problems.

On page 253 a page from another book has been inserted! Several other less spectacular technical defects mar the book: on page 19 the egg is shown ready for "transplantation" on the wall of the uterus; a citation to Burch is correctly spelled in the literature, though in the text the name is consistently *Birch*.

What is the balance of account? Despite the defects of this book from a social standpoint, there can be no doubt that it stresses a point of view much needed in American sociological thinking, and which, if generally adopted by sociologists, would round out, enrich, and balance the point of view which they commonly present on social problems. Long study of the history of ideas convinces me that sociologists tend to stress unduly social conditioning as geneticists tend to stress genetic causation. However, if I had to choose between the kind of integration represented in this book and the kind of integration so frequently met with in some sociological textbooks I should choose the former. But most sociologists would not agree with me.

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ALFRED GROTJAHN AS A EUGENIST

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ALFRED GROTJAHN* is the founder of the German social hygiene movement. In the United States "social hygiene" is virtually synonymous with venereal disease control, but in Germany a much broader meaning is placed on the term. In Grotjahn's own words: "Between man and nature there is culture, and therefore, the physical-biological point of view in hygiene must be enlarged by social considerations." This concept called for a method of inquiry through the social sciences to determine the conditions under which a truly hygienic culture could be universalized among the present population as well as in future generations. And the concept was held to include an investigation of means whereby such a culture could be brought to pass. Mere knowledge was held not to be enough. Through culture in the anthropological sense we must make this knowledge come alive to guide and protect the youth of this and coming generations.

Grotjahn never doubted that there was a close connection between social hygiene and eugenics. First of all social hygiene should not confine itself to the attempt to universalize the hygienic culture of the present generation, its goal being a wider one, viz. "the eternal youth of the nation." He was convinced that only this branch of science would ultimately be able to solve the puzzle of why some of the great civilized nations had vanished from the earth. Out of these investigations the science may devise means which will enable a people, provided they observe such rules in their customs, law and administration, to retain permanently and intact the physical basis of their culture and of their national power.

The above lines might lead the reader to suspect that there is no great difference between Grotjahn's views and the present German outlook. A brief analysis of the latter will show, however, that that is not the case.

The initiative of the present German race-theory was taken by Ludwig Schemann, who in 1894 founded the "Gobineau-Association" and in 1898-1901 published the German translation of Gobineau's major opus *Essais sur l'Egalité des Races Humaines.* In his native country Gobineau's work, published in the years 1853-55, was soon entirely forgotten, but the Germans brought him a posthumous fame. The French Count Arthur Gobineau tried to prove that equality is against nature, which created men of two kinds—superior and inferior, the former belonging to the Nordic race. To add to the irony of history, an Englishman, Huston Stewart Chamberlain, published a monograph in Germany in 1899 (*Grundlagen des XIX. Jahrhunderts*) pointing out the weaknesses of the Western liberalism and contrasting it with aristocratic Nordic-race thought. These two pseudo-scientific works can be considered as foundation-stones of the present German race-theory. The chief proponent of this theory has become Hans Guenther who still plays his part in Germany.[10]

The founder of the German eugenics movement is Alfred Ploetz who in 1895 published a monograph, *The Elements of Race-Hygiene*[20] and who, in 1904, established the still leading periodical *Archiv fuer Rassen- und Gesellschafts-biologie.* The basis of the Ploetz School is apart from the race-theory, an unconditional adoption of the Darwinian concept of the survival of the fittest. In connection with the latter there should

*For biographical information about Grotjahn the reader is referred to his autobiography,[9] and to Robson's account.[21]

be mentioned another pioneer of German eugenics, Wilhelm Schallmayer,[22] who energetically opposed the Nordic race theory.

The basis of the leading school of eugenic thought in Germany is roughly as follows: There is a marked difference in physical appearance of the people of various nations (and even between strata within a nation) owing to varied gene-constitution, and this also is reflected in different hereditary abilities in making progress in civilization among the races (and among the strata within the races[18]). Crossing of races leads to sterility, degeneration, the decline of civilization; only selection is able to influence physical and mental change in the race—hence neither public education nor any other environmental measures are of any use in this connection. There is no heredity in acquired features; in order to retain a healthy race the struggle for existence as described by Darwin is necessary. Hence,—an expurgation of all inferior elements of the race is essential and thus any social reform, and, particularly, social welfare must be considered as dysgenic.

Grotjahn was opposed to such a conception of eugenics from the outset. In his opinion any deduction from Darwin's theory was incompatible with the principles of eugenics. He held that the application of this theory was limited entirely to zoology and botany, and believed that only the inductive method was useful to eugenics. He also resisted the notion of race-hygiene because of its connection with Gobineau's Aryan theory and because of its general subjectivity. For him the science of eugenics should not depend in its application on any race valuation, or refer at all to any race: it must be fair, objective and of service to any group.

With regard to the problem of the allegedly dysgenic influence of social reform and of social welfare, he published a monograph as early as 1904 on the problem of degeneration,[5] in which he questioned whether the universalization of hygienic culture leads inevitably to degeneration, because such measures unavoidably maintain or even to some extent increase of inferior elements. As a result of his investigations, he concluded that the greater part of degenerative phenomena is caused by environmental conditions. Thus social-hygiene measures were rather to be considered as prophylactics against the degeneration caused by environmental conditions. Until all possible social measures are exhausted he was skeptical about positive results from eugenics.

That Grotjahn was right in his reasoning, we can see from the elaborate studies by the German hygienist Ferdinand Hueppe.[12-14] Hueppe's background and his outlook was based entirely on natural science; he had, as Director of The Institute of Hygiene at the German University in Prague, an ample opportunity to experiment; he is one of the founders of the science of constitution, and of the constitution-hygiene movements. Since his attitude towards the Nordic race theory is not unsympathetic, he is not biased in favor of an environmental theory. From his studies he concluded that environmental conditions do influence the cell-plasma. For instance, according to his theory, any infectious disease is the result of a multitude of functions which can be divided into three groups: (1) the infective organism, (2) the hereditary constitution, and (3) the environmental conditions, such as nutrition. Hence he not only advocated social reform and social welfare but also strongly urged an intensive study of the hygienic culture in order to make such measures effective. He was against any selective method based on a chosen class, being a supporter of eugenic methods for all strata of the population.

Again, in order to corroborate Hueppe's theory we must study anthropometrical data or still better, mortality statistics of children born during the World War. We deliberately confine ourselves to English statistics, because England's position during the war was more favorable than other European belligerent countries. Yet, the statistics of tuberculosis mortality in England and Wales according to sex and age show

that the steady long-term decline of tuberculosis mortality is not manifested among female adolescents born during the war. These have a higher tuberculosis rate than those born before or after the war. At first this reversal of trend was explained as due to a selection of youth, but since the report for 1935 this exception has been recognized as having been the effect of unfavorable conditions at the time when the children were infants. Furthermore many investigations demonstrate that when expectant mothers do not get sufficient food the newborn children not only show size and weight decline below the average but also an insufficient power of resistance toward infectious disease.[15, 16] In view of the above the recent population-policy of the Nazi regime, urging every healthy woman to bear a "war-child," is unwise because of food restrictions now in effect in Germany.

With regard to the relation of the nutrition problem to eugenics Grotjahn pointed out in his early work on nutrition[7] that the chronic under-nutrition of an underprivileged urban population must eventually lead to degeneration. The effect of the war on tuberculosis rates cited shows that he was right in his assumption. But the eugenists of the race theory school seem to be ignorant of this fact as the above quoted policy shows. Moreover, they repeatedly refer to lower intelligence quotients of the children of the poor as compared with those of wealthy children in order to prove the consistency of the selective theory.[18, 19] To disprove this we may refer to practical Britishers, viz. to a resolution by the Departmental Committee on Physical Deterioration, 1904:

> To educate underfed children is to promote deterioration of physique by exhausting the nervous system. Education of underfed is a positive evil. Between the ill-fed child there is a difference not only in physical condition but also in educable capacity. (19 p. 327, see also page 340.)

This some evidence shows that in the controversy between Grotjahn and the race-hygienists the former was right that eugenics is of no use unless other social-hygiene measures were fulfilled.

Many German writers on race-theory and race-hygiene express a doubt as to the positive value of modern civilization, since the latter weakens selective processes, Grotjahn has shown that there was no ground for such pessimism: it was true that urbanization and industrialization has caused many ill effects; but it was also true that many social reforms in industrial countries have brought a higher standard of living, higher education, etc. The sum of the good effects he believed was greater than the ill effects. Grotjahn was greatly concerned by the steadily declining birth-rate among civilized people. He devoted the last twenty-five years of his life to questions of population, the subject of the greater part of his writings. His anxiety concerning the effects of the decline of the birth-rate was so great that he often expressed an opinion that the question of the quantity of population must at present be preferred to that of the quality. At the beginning of the present century he predicted that as the cholera-epidemic in the Nineteenth Century was the stimulus for the science of hygiene, the decline of the birth-rates in the present century will become the stimulus for eugenics. To Grotjahn the population question was inseparable from eugenics. For this reason in a paper read before the Sociological Society in Vienna in 1908 he strongly recommended a close collaboration between physicians, biologists, economists, and sociologists. "Such a collaboration," he said, "is very useful for the solution of many problems, but it is absolutely necessary when dealing with the problem of degeneration." He believed that the best way to foster such a collaboration would be for every expert to work independently in his own field unless a scientist should command methods in both fields, in which case he must deal with the problems separately*—at times from

*It is interesting to note that the movement of "Unity of Science" suggests similar tactics (See "International Encyclopaedia of Unity of Science." Chicago University Press.)

the standpoint of natural science, at times from the angle of social science. In this paper he advocated for the first time the project of insurance of parenthood, a project to which he clung the rest of his life.*

In order to avoid the danger of the decline of the population, he advocated that: (1) Every couple should bring up at least three children; (2) No parents should be free from such a duty even though there be minor imperfections in their heredity. In such cases the minimum should be also the maximum; (3) Those parents whose hereditary constitutions are good beyond any doubt should have the right to produce more than three children and an allowance should be paid to the parents for every child from the fourth onward. Such allowances would be collected from the unmarried people and from those couples who have no children or less than three: Unmarried people should pay the full tax, childless couples two-thirds of the tax, those with one or two children one-third. Those with three children are to be free from tax, and those with good hereditary background with four children receive an allowance of two units, and for every child after the fourth, one unit each—until maturity.

He defended his suggestion that couples with minor hereditary inferiority should also produce children by reasoning that our knowledge of human heredity is still insufficient to determine which couples in this category should not have any issue. Only in those cases where no reasonable doubt existed that defective children would be produced should reproduction be discouraged.

Although Grotjahn was concerned about the quantity of population he nevertheless advocated birth-control, but on the condition that contraception should be wisely used and not abused as it has been since the end of the Nineteenth Century. He even went so far as to be the only professor in Germany who demonstrated means of birth-control before his students. In his opinion every practitioner was duty-bound to be acquainted with contraceptive techniques.[3] To him birth-control meant merely a rational population policy among a healthy, highly-qualified population and not a means to produce a declining population. In addition to this main reason for advocating birth-control he has two other reasons: he was opposed to too many births by one mother and he advocated an interval between children of at least two years for the sake of the mother's health. Generally speaking, he accepted the proposition that every child should be born as a result of a decision to have a child rather than by mere chance. He also wanted to prevent abortions. He favored legalization of abortion only for medical or eugenic reasons, and even then only in a public hospital after a decision by a committee of health officers. Only a few months before his death he was persuaded to concede that legalization to include social reasons might be necessary. His change of mind in this respect was brought about through records sent to him by a physician in a small town who performed about 500 abortions during one year.[6a] These case histories convinced Grotjahn that an economic crisis might have to be considered to be as serious as medical or eugenic factors.

He was opposed to compulsory sterilization, but advocated the creation of humanely conducted asylums for those of poor physical or mental constitutions.[4] Such asylums should be created and managed so that the patients should live in small settlements having an atmosphere not of institutions, but of homes. The inmates should have every possible freedom except that of producing issue.

With regard to mental instability Grotjahn sometimes expressed the opinion that not all psychopaths should be considered dysgenic. His son, a talented psychiatrist, has published Grotjahn's pedigree[9] which clearly shows that according to the present Racial Law in

*His last work on this question appeared as a paper, prepared (but not read) for the International Population Congress in Rome, in 1931.

Germany, Alfred Grotjahn's parents would have been sterilized. This pedigree is a most interesting document, which Grotjahn himself used as a demonstration in his classes on eugenics, and he himself openly admitted that he had inherited a psychopathic constitution:[2] "There is no doubt: Were I a child of poor people I would have become a criminal . . ."[2] But on the other hand a strong will and an energetic determination to go the way chosen by him coupled with favorable social and economic conditions during his childhood, made him one of the most interesting scientists of his time.

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GENETICS, KINSHIP AND GENEALOGY

CERTAIN applications of genetic theory to human kinship have never been clearly made. Such applications lead to conclusions, which, although not startling to geneticists, are not generally known and their significance has never been made clear to the general public. I was reminded of this fact by news items coming out at the time of the visit of the British King and Queen to this country last summer. Various reports stated it had been found that there was a distant kinship tie between the Queen and certain American presidents. One version said she was related to George Washington, while the other said she was an eighth cousin twice removed of President Roosevelt. Whether one or both are authentic makes little difference for neither has any biological or genetic meaning except, possibly, that the Queen and the president referred to are of the same racial group.

It can be shown by known facts of gene assortment that one brother may be more kin to another than to a third, that we are more kin to some grandparents than to others and that some far removed grandparents are even *empty ancestors* from whom we inherit nothing at all.

Genes are carried in the chromosomes at specific loci. Man has 24 pairs of chromosomes. One of each pair is lost, apparently by chance, at the reduction division of the developing gamete. If inherited characteristics are determined by genes then "blood kinship" is based on the inheritance of like genes from a common source. Two out of three full brothers may inherit more chromosomal parts, that is to say more like genes, in common with one another than with the third brother and, therefore, may be said to be more kin to one another than to the third by the difference of some allelomorphs. Identical twins inherit the same genes. The closeness of their relationship, contrasted to the differences between them and other offspring of the same parents, is an extreme example of this situation. This argument would be nullified and genic differences with respect to the germ line would be transitory if it were not for irreversible mutations. Otherwise, the potentialities of genes inherited from different brothers would be the same in the long run, due to reversible mutations doing away with the differences of allelomorphs.

If there were no crossing-over no one could inherit chromosomes from more than forty-eight ancestors in any one generation. Ten generations back we would have 1,024 ancestors, from a maximum of only forty-eight of whom we inherit anything. The other 976 would be only the physical mates of our true ancestors and to them we would be actually no "blood kin" by direct inheritance However, inbreeding takes place and a few generations back the real number of ancestors becomes less than the theoretical number. About thirty generations back the theoretical number of ancestors becomes greater than the population of the earth has ever been. Inbreeding decreases the chances of occurrence of empty ancestors and crossing-over decreases them still more, but does not do away with them entirely; so it is possible to have ancestors from whom we have inherited not a single gene, that is to say nothing at all. This fact may serve as a solace to some for it means that people are not necessarily related to every skeleton hanging in the family tree. Of course, the converse situation also holds and one cannot depend on being related to distant notables either in the direct ancestral or collateral lines.

The study of genetics and eugenics has shown the importance of immediate ancestry. Considerations like the above lead to the paradoxical conclusion that distant ancestry or kinship means practically nothing. In the days of the Old South it was said that to qualify as a lady a woman had first to know the maiden names of her four great-grandmothers. Further than this it is probably useless to go at any time and long-range genealogy becomes an interesting pastime, of historical interest, but having no hereditary significance.

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Rockport, Texas

Genetics
And the
Origin of Species

BY THEODOSIUS DOBZHANSKY

THIS important book has been a recognized classic in its field ever since it was published. It is now more widely used than ever as a text, and its value as an original contribution to the subject is acknowledged everywhere. What these reviews have said about it constitutes not only the general critical opinion, but also gives the best idea of what it contains and how it can be of use to you:

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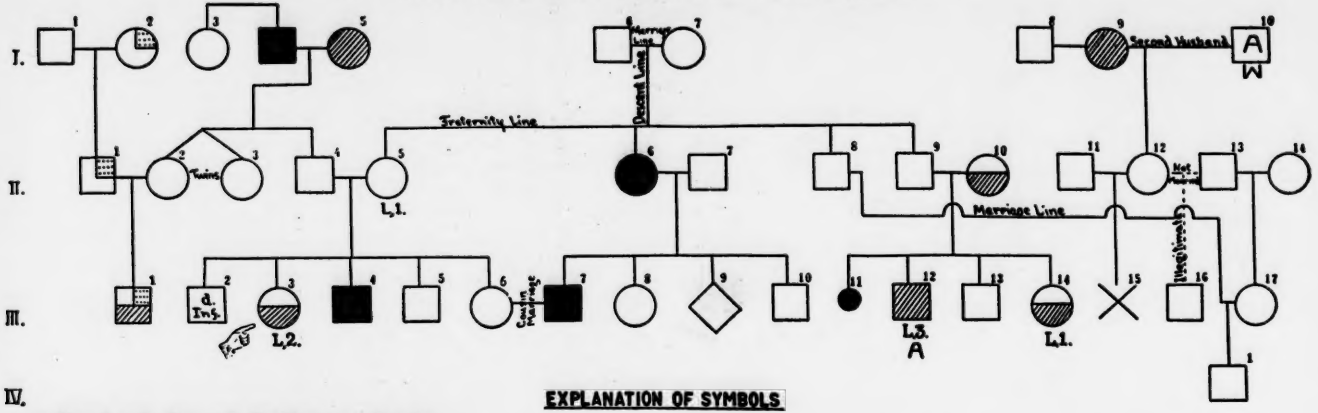
SAMPLE PEDIGREE CHART SHOWING THE MANNER OF CONSTRUCTION, AND THE USE OF STANDARD AND SPECIAL SYMBOLS.



EXPLANATION OF SYMBOLS

(a). Standard for all Pedigree Charts:

□=Male; ○=Female; ◇=Sex unknown; ●=Still-birth or Miscarriage; X=Children — number and sex unknown; ○○=Twins; d. Inf.=Died in Infancy; ☞ Points to the Propositus or central figure in the pedigree.

□—○=Parents.

□○⌐○○=Children.

Roman figures to the left indicate generations; Arabic figures locate individuals, (thus III. 7 is the young man in the third generation who married his cousin).

The following letters, placed in or around the individual's pedigree symbol, are standard for certain traits: A, alcoholic; B, blind; D, deaf; E, epileptic; F, feebleminded; I, insane; M, migraineous; N, normal in reference to traits under consideration; Ne, neurotic; P, paralytic; Sx, sexually immoral; S, syphilitic; T, tuberculous; W, wanderer.

(b). Especially devised or selected for this particular pedigree-chart:

■ ● = Successful leaders in politics. ◱ ◔ = Extra thumb on right hand. L.1. = Highly successful author. L.3. = Little or no ability in literary efforts. ▨ ◍ = Superior in vocal music. ▨ ◍ = Medium attainment in vocal music.

To fit the particular family and traits (whether physical, mental or temperamental; good or bad) under consideration, invent special symbols, or select special letters (in addition to those standardized under (a) above) to be placed within or near the particular individual's pedigree symbol, to indicate particular traits and their degree of development.

Prepared by the International Commission of Eugenics. See *Journal of Heredity* for August 1926. Additional copies may be obtained from A.G.A.